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Analytical and Critical Reviews.

Review I.

1. *Medicinskt Archiv, utgifvet af Lärarne vid Carolinska Institutet i Stockholm.* Redigeradt af E. A. Key, Professor i Pathol. Anatomi; C. J. Rossander, E. o. Professor i Chirurgi; A. Kjellberg, Adjunct i Pædiatrik. Tredje Bandet, Tredje Häftet. Med 6 Träsnitt, och 6 plancher.

Archives of Medicine, published by the Teachers in the Carolinean Institute at Stockholm. Edited by E. A. Key, Professor of Pathological Anatomy; C. J. Rossander, Professor Extraordinary of Surgery; and A. Kjellberg, Assistant in Pædiatrics. Third volume, third number. (With six woodcuts and six plates.) Stockholm: Samson and Wallin. 1867. 8vo, pp. 230.

A. *Bidrag till kännedomen om Tungans Smakpapiller.* Af Christian Loven (Med 1 planche). *A Contribution to our Knowledge of the Gustatory Papillæ of the Tongue.* By Christian Loven, Prosector in the Carolinean Institute. (With one plate.)

B. *Om Ovariotomiens tidigare historia i England och Amerika, och operationens nuvarande ståndpunkt.* Af Sven Sköldberg .(Med 6 träsnitt).

On the early history of Ovariotomy in England and America, and the present position of the operation By Sven Sköldberg. (With six woodcuts.)

c. *Fall af Encephalitis Corticalis et Hydrocephalus Acutus.* Meddeladt af Dr. **Ernst Oedmansson**, Adjunkt i Pathol. Anat. vid Carolinska Institutet.

Case of Encephalitis Corticalis et Hydrocephalus Acutus. Communicated by Dr. **Ernst Oedmansson**, Assistant in Pathological Anatomy in the Carolinean Institute.

d. *Om Trikinernas naturliga förekommande.* Af **Axel Key**, Professor i Pathol. Anat. vid Karolinska Institutet.

On the natural occurrence of the Trichinæ. By **Axel Key**, Professor of Pathological Anatomy in the Carolinean Institute.

e. *Studier öfver Lifmodrens Byggnad hos Menniskan.* Af **Hjalmar Lindgren**, Med. Lic. (Med 5 plancher.)

Studies respecting the Structure of the Uterus in the Human Subject. By **Hjalmar Lindgren**, Licentiate in Medicine. (With five plates.)

f. *Berättelse om Undervisningen vid Karolinska Medico-Kirurgiska Institutet under läs-året* 1865—1866.

Report of the Instruction given in the Carolinean Medico-Chirurgical Institute during the scholastic year 1865—1866.

2. *Om Hæmoptyse, navnlig den lethale, i anatomisk og klinisk Henseende.* Af Dr. **Vald. Rasmussen**. 'Hospitals-Tidende.' Kjöbenhavn. 1868.

On Hæmoptysis, especially when fatal, in its anatomical and clinical aspects. By Dr. **Vald. Rasmussen**. From the 'Hospitals-Tidende.' Copenhagen. 1868.

3. *Cholera i Christiania i* 1866. Beretninger til Sundhedscommissionen. Af H. **Steffens**, O. **Lund**, **Lochmann**.

Cholera in Christiania in 1866. Reports to the Sanitary Committee. By H. **Steffens**, O. **Lund**, and **Lochmann**. Christiania: C. Werner and Co. 1867. 8vo, pp. 80.

4. *Den medicinske Skole i Reykjavik.* (Særskilt Aftryk af 'Ugeskrift for Læger.') 3 Række, V. Nr. 13, 14. 1868.

The Medical School in Reykjavik. (Reprint from the 'Ugeskrift for Læger.) Third series, V. Nos. 13, 14. 1868. 8vo, Pp. 31.

5. *Et Tilfælde af Kyphosis lumbo-sacralis med Forsnevring af Bækkenets Apertura inferior.* Meddelt i det kgl. medicinske Selskab, Februar, 1868. Af A. **Stadfeldt**. (Med 2 lithograferede Tegninger.) 8vo. Pp. 22.

A Case of Lumbo-sacral Cyphosis with Constriction of the Inferior Aperture of the Pelvis. Read before the Royal Medical Society of Copenhagen, February, 1868. By A. STADFELDT. (With two lithographed drawings.)

6. *Helbredelsesanstalter.* I. Den kgl. Födselsstiftelse, fra 1ste April, 1866, til 31te Marts, 1867. (Overakkoucheur, Dr. STADFELDT.)

Sanitary Institutions. I. The Royal Lying-in Institution, from the 1st April, 1866, to the 31st March, 1867. Accoucheur-in-chief, Dr. STADFELDT.

7. *Nogle Bemærkninger om Tegnene paa en overstaaet Födsel i mediko-forensisk Henseende.* Af Dr. STADFELDT. 'Ugeskrift for Læger,' 9 Mai, 1868.

Some remarks on the Signs of Delivery having taken place, in a medico-forensic point of view. By Dr. STADFELDT.

8. *Om de hygieniske Forholde vedkommende Födsel og Barselseng med særligt Hensyn til Resultaterne i flere af Udlandets Födselsstiftelser.* Ved Dr. F. C. FAYE, Professor i Födselsvidenskab, Fruentimmer- og Börnesygdomme ved Universitetet, Overlæge ved Födselsstiftelsen og Börnehospitalet i Christiania. Christiania: hos C. C. Werner og Co. 1868. 8vo. Pp. 84.

On the Hygienic Conditions relating to Parturition and the Puerperal State, with special reference to the results obtained in many of the Foreign Lying-in Institutions. By Dr. F. C. FAYE, Professor of Midwifery and Diseases of Women and Children in the University, First Physician to the Lying-in Institution and Children's Hospital in Christiania.

9. *Physiologiska undersökningar öfver Dufvans Magar.* Af FRITHIOF HOLMGREN. Upsala Läkareförenings Förhandlingar. Andra Bandet. Åttonde Häftet. Upsala: W. Schultz. 1867.

Physiological investigations respecting the Stomachs of the Pigeon. By FRITHIOF HOLMGREN. 'Transactions of the Medical Society of Upsala. Volume II., Part 8.

10. *Försök öfver dietens inflytelse på Magen.* Af FRITHIOF HOLMGREN. (Aftryck ur Upsala Läkareförenings Förhandlingar.)

Experiments upon the influence of diet on the Stomach. By FRITHIOF HOLMGREN. (Reprint from the 'Transactions of the Medical Society of Upsala.') 8vo. Pp. 19, with a plate.

11. *Ueber das Epithel der Maculæ acusticæ beim Menschen.* Von Dr. M. V. Odenius, Prosector in Lund. (Separatabdruck aus M. Schultze's 'Archiv f. mikr. Anatomie,' Bd. III. 1867.)

On the Epithelium of the Macula acoustica in Man. By Dr. M. V. Odenius, Prosector in Lund. (Reprint from M. Schultze's 'Archives of Microscopical Anatomy,' Vol. III, 1867.) With a plate.

12. *Om Vestibular-säckarnes form och läge i menniskans öra.* Af M. V. Odenius. (Föredraget i Physiographiska Sällskapet i Lund den 13 Mars, 1867.)

On the form and position of the sacs of the Vestibule in the Human Ear. By M. V. Odenius. (Read before the Physiographical Society of Lund, March 13, 1867.)

1. The Swedish 'Archives of Medicine' which we have placed at the head of the foregoing list, improves with every number, and may now challenge comparison with any journal, whether as regards the extent and importance of its articles, or the beauty of its typography and illustrations. From the notices which we have given of previous issues, our readers are aware that this publication differs from periodicals properly so called, in not appearing at stated intervals, although two or three numbers are usually given yearly, and in the greater length and more elaborate nature of some of the essays it contains. Each essay is separately paged, and when illustrated, is immediately followed by its plates, so as with ease to form, if desired, a separate work.

A. The subject of the first essay in the number before us, the gustatory papillæ of the tongue, is one which has already at some length engaged our attention, in considering the work of Axel Key in our twenty-ninth volume, page 422, and of Dr. Engelmann, of Utrecht, in the number of this Review for July, 1868. Their researches related, however, to the tongue of the frog, while Hr. Lovén has investigated the organ in the mammalia. The author sums up his paper, which is illustrated with a most beautifully executed plate, in the following words:

"From what has gone before, I must consider it most probable, that in the papillæ circumvallatæ in the calf (and in man) the gustatory nerves, after losing the medullary sheath in the outer layer of the mucous membrane, are continued up into the gustatory bulbs as naked axis-cylinders, and similarly circumstanced divide into a number of branches, which pass directly into the gustatory cells. But how far the gustatory cells, which in this manner come to be connected with the same nerve filament, belong to one or to several gustatory bulbs, or whether both these conditions may exist, I cannot decide.

"On comparison with the results at which Key arrived on examining the gustatory papillæ of the tongue of the frog, much agreement is to be found in the arrangement I have now endeavoured to describe, but also some difference. Especially is this the case with the connexion between the nerves and the gustatory cells, and particularly the central outrunners of the latter. In the frog, in fact, the axis-cylinders are resolved, after they have emerged from their medullary sheaths, into a great number of the most minute varicose filaments, which at their extremities bear the gustatory cells, while on the contrary in the calf the connexion between the nerves and the gustatory cells, to judge from their central outrunners, is effected through somewhat thicker, only sparingly and irregularly varicose filaments, which have the same appearance as isolated axis-cylinders.

"Lastly, as to the fungiform papillæ in the calf, the imperfect investigations I have instituted with respect to them have afforded at least this result, that also in them, although much more sparingly, gustatory bulbs and gustatory cells occur, in which, however, the remarkable dissimilarity in their arrangement prevails, that while in the papillæ circumvallatæ they occupy a certain, well-defined zone, namely, the sides of the papilla looking towards the surrounding furrow, and to a certain extent protected by the latter, they are met with in the fungiform papillæ, scattered without order between the small papillæ on the upper free surface, as may be easily ascertained by examining a thin superficial section from the summit of such a papilla, which exhibits the gustatory bulbs corresponding to the hole in the outermost layer of the epithelium."

B. We have next an elaborate and interesting article, extending to 127 pages, by Dr. Sven Sköldberg, on the early history of Ovariotomy in England and America, and on the present position of the operation.

The accounts, observes the writer, of the removal of the ovaries in women, reach far back in history.

Thus it is stated of the Lydian King Adramytes who lived in the sixth century before Christ, "that he was the first who castrated women, and employed them in the place of male eunuchs."[1] And this custom is said to have been afterwards general at the eastern court.

Boerhaave mentions a story of a sow-gelder in Germany, who was so incensed at the unchastity of his daughter, that he opened her side and castrated her, relieving her thenceforward from all sexual desire.[2]

The first known case, which can be called ovariotomy, according to the more limited signification which must now be attached to this word, is that of an operation performed in the year 1701, by Dr. Houstoun, of London, The author translates the case *in extensó* from the 'Philosophical Transactions,' vol. xxxiii, No. 381, p. 8, London, 1726. It is there entitled "An Account of a Dropsy in the left Ovary of a Woman, aged 58, cured by a large incision made in the side of the Abdomen, by Dr. Robert Houstoun." The patient survived the operation, which was evidently a case of partial ovariotomy, in perfect health for upwards of thirteen years. We may take this opportunity of assuring Dr. Sköldberg, that his translation of "it drew towards a point," as given in the text, about which he expresses some doubt in a note, is quite correct.

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[1] 'Athenæi Naucratitæ Deipnosophistarum, Libri quindecim.' Illustravit Johannes Schweighäuser, Anno xii (1804), tom quartus, p. 415.

[2] Hermanni Boerhaave 'Prælectiones Academicæ;' edidit Albertus Haller. Gottingæ, 1744, tom. v, pars. ii, 126.

Extirpation of both ovaries was accidentally performed in the middle of the eighteenth century, by Percival Pott, Surgeon to St. Bartholomew's Hospital in London.[1]

We find generally stated as the first ovariotomy, a case by L'Aumonier in Rouen, who in 1781 extirpated an ovary, which was accidentally met with in opening a pelvic abscess.[2] The author, however, having quoted the case at length, remarks that—

> "It is evident from the foregoing that we cannot reasonably refer this case to the category ovariotomy according to the meaning which at present attaches to that operation, for then King Adramytes' castrator might with equal justice be called the father of ovariotomy; and if we deny this because the extirpation of the ovaries was not a surgical operation undertaken for a curative object, under those circumstances if we will not recognise Houstoun's as the first, because it was only a partial extirpation, Percival Pott was undoubtedly the first ovariotomist."—p. 8.
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> "Dzondi has also been mentioned as one among the first ovariotomists; but it will be sufficient to indicate that this so-called ovariotomy, which, moreover, was performed much later than many of the unquestionable operations, was undertaken upon a boy.
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> "America is the country which with justice claims to be regarded as the cradle of ovariotomy, and to Dr. Ephraim M'Dowell, of Kentucky (born in Virginia, November 11, 1771, died at Danville, June 25, 1830), belongs the honour of being the first surgeon, who not only after previous diagnosis undertook the first complete extirpation of a dropsical ovary, but also performed a whole series of these operations."—p. 9.

Dr. Sköldberg refers, only to refute it, to the statement recently put forward, that the honour belongs not to Dr. M'Dowell, but to a sow-gelder named John King.

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[1] 'The Chirurgical Works of Percival Pott.' New edition. London, 1783, vol. iii, p. 329.

[2] 'Observation sur un depôt de la trompe et sur l'extirpation de l'ovaire;' par M. L'Aumonier, Chirurgien en chef du grand hôpital à Rouen, et Correspondant de la Société. See "Histoire de la Société Royal de Médecine;" Annécs 1782 et 1783, vol. i. Paris, 1787, p. 296.

Dr. M'Dowell's first operation was performed in December, 1809, on the person of a Mrs. Crawford, who survived the operation thirty-two years, had no relapse, and enjoyed excellent health until her death, which occurred on the 30th March, 1841, at the advanced age of seventy-eight years.

The total number of Dr. M'Dowell's operations was thirteen or fourteen. The first five are reported by himself. Of these, the fifth only was fatal. Of four additional cases recorded by others, one, likewise, was fatal. As to the remaining cases, it is supposed "that he was not so fortunate in his later as in his earlier operations."

Contemporaneously with Dr. M'Dowell's later operations, ovariotomy was performed by Dr. Nathan Smith, Professor of Surgery and of Theoretical and Practical Midwifery in Yale College, Connecticut. The patient recovered. The method adopted by Dr. M'Dowell was that afterwards called in England the "major operation," opening the whole abdominal cavity from the sternum to the os pubis, so as to take out the tumour in its integrity. The mode employed by Dr. Smith was that subsequently named the "minor operation," consisting in so far diminishing the volume of the cyst by tapping, that the tumour may be extracted through a comparatively small opening in the abdominal wall.

Another novelty in Professor Nathan Smith's operation was the separate tying of each bleeding vessel, and the employment of the lost ligature, instead of Dr. M'Dowell's tying in mass and leaving the end of the ligature outside the wound.

A third novelty was the ligature of leather, the advantage of which was stated to be that as an animal substance it would be absorbed, and not remain as a foreign body in the abdomen.

The author now transfers the scene of his history to Europe.

When Dr. Ephraim M'Dowell in 1817 published his first three cases in the 'Philadelphia Eclectic Repertory,' he sent a copy of this journal to his former teacher John Bell, of Edinburgh, whom he had heard in his lectures descant upon the hopelessness of the then treatment of ovarian diseases, and the possibility of the radical cure of the latter by extirpation. John Bell was staying at the time in Italy, and Mr. Lizars, who had charge of his official correspondence, thus became possessed of the report. Mr. John Lizars, Lecturer on Anatomy and Surgery in Edinburgh, was clear-sighted enough to see the importance of Dr. M'Dowell's communication, and had sufficient courage to defy the prevailing prejudice, and himself to perform the new operation. Rightly to estimate his merit in this respect, the horror then generally entertained of any injury of the peritoneum must be borne in mind. One man had, however, in clear and unmistakeable words appeared as a champion for the operation, and that man was John Hunter. This distinguished surgeon had in his lectures on the principles of surgery delivered in 1786-87, shown that the treatment of hydatids of the ovaries, as cystic degeneration of those organs was then called, was only palliative, and that if met with in an early stage the cyst ought to be extirpated. "There is no reason," he added, "why a woman should not bear castration as well as other animals. The operation would consist simply in opening the abdomen, which we often do in healthy subjects without inconvenience."[1]

A full account of Lizars' first four operations was published by him under the title of "Observations on Extraction of Diseased Ovaria, by John Lizars, Edinburgh, 1825." He concludes his essay by remarking, that from these cases it may be inferred, that the danger of opening the abdominal cavity is not nearly so great as Morgagni, and surgeons in general have imagined, and that in dropsy of the ovary postponement of the operation is more dangerous than the operation itself.

The results of his operations were, however, but little encouraging. In two cases no tumour at all was found, and one of these patients died; in two, the disease was fibroïd of the uterus; in one of these the operation was given up; and in the other, where the tumour was removed, the patient died. Of the two cases in which the diagnosis was correct, in one the ovarian tumour was removed and the patient died; and in the other, where the patient survived the operation, a degenerated ovary certainly was taken away, but the operator was compelled to leave the other, which also was diseased.

The cases were, however, badly selected. Of the three women who died one was a drunkard, and another an opium eater, circumstances which, if known beforehand, ought to have prevented an operation being undertaken.

The author describes, in vivid language, the storm of wrath which burst over the heads of the advocates of the operation. The most violent and most unmerciful assailant was Liston, Lecturer on Surgery in Edinburgh.[2] Syme was not less decidedly opposed to it ('Principles of Surgery,' second edition, Edinburgh, 1837, p. 374), and though in the latest edition of the same work, published in 1863, he acknowledges that "the operation has now become established in practice," he has never been induced to perform it himself. Sir William Lawrence, Professor Hamilton, and Dr. Mackintosh, were all opposed to it.

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[1] 'The Works of John Hunter,' edited by James F. Palmer. London, 1835, p. 573.

[2] 'Elements of Surgery,' by Robert Liston, Surgeon to the Royal Infirmary, Edinburgh; Lecturer on Surgery, &c. &c. Part third. London, 1831, p. 54.

There was, however, one who appreciated the value of the new operation, and had independence enough to come forward as its advocate in the midst of the loud and general indignation produced by the results of Lizars' operations. This was Dr. James Blundell, Lecturer on Physiology and Obstetrics at St. Thomas' and Guy's Hospitals, who has lived to see the complete ratification of his opinions.

> "Little encouraging as Lizars' operations may seem, they, nevertheless, contributed in an essential degree to the general introduction of ovariotomy. For besides having incontestably shown that the operation was not so absolutely fatal as had been supposed, Lizars' had actually successfully extirpated an ovarian cyst, a fact which the opposers of the operation could scarcely get over; but, above all, his operations had directed the attention of the medical world to the question, and we must do the English surgeons the justice to say, that though they had not light to comprehend the value of the new operation, they did not receive it with the haughty and fatal silence with which the American surgeons treated M'Dowell's case. The violence with which the operation was attacked excited an interest in it, and the question was kept alive, and even Liston may thus be regarded as one of the promoters of ovariotomy.
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> "It was not long until Lizars' example was followed; and, although rejected and condemned by legitimate surgeons, the operation was repeated in many places both in England and America."—p. 41.

"The first attempt at ovariotomy in London was made in July, 1826, by Dr. Augustus Bozzi Granville, by birth an Italian, who after having taken his doctor's degree, had settled in London, where he still lives." Messrs. Brodie (afterwards Sir Benjamin), Keate, Earle, &c., were present at the operation. When the abdomen was fully opened, it was ascertained that the tumours were adherent to the abdominal wall and to some loops of intestine. It was, therefore, determined to proceed no further, and the incision, which was seven and a half inches in length, was closed. The patient did well.

Dr. Granville operated on a second patient on the 21st March, 1827, when he removed a tumour weighing about eight pounds and as large as a man's head, through an incision nine inches in length. The patient died on the third day of exhaustion caused by venesection injudiciously performed by an over-zealous assistant.[1]

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[1] 'London Medical Gazette,' 1842–43, vol. i, p. 540.

In 1829, gastrotomy was performed in London by an unknown operator. The case is reported by Dr. Gooch.[1] The patient's health sustained much injury.

In America, Dr. Alban G. Smith (also called Dr. A. Goldsmith), performed ovariotomy three times. Two of the patients recovered. One died of secondary hæmorrhage.[2] The first of these operations was performed on the 24th May, 1823.

In 1827, Dr. A. Trowbridge attempted ovariotomy. The extirpation could not be completed in consequence of adhesions. The patient recovered.[3] Dr. Trowbridge subsequently operated by mistake in a case of chronic peritonitis with effusion. The patient recovered perfectly.[4]

In 1828, ovariotomy was attempted by Dr. R. D. Mussey. When the abdomen was opened it was found that the very large sac was so firmly and extensively attached that its removal was out of the question. Four or five pints of somewhat turbid fluid were drawn off with a catheter, and inflammation of the inner surface of the cyst was induced by the introduction of a tent. At the end of a few weeks not a trace of the sac could be discovered. About a year after the operation the patient gave birth to a son, the fourteenth and last child.[5]

Complete ovariotomy, with a favorable result, was performed in 1829, by Dr. David L. Rogers, Lecturer on Surgery in New York.[6]

A solid tumour, proceeding from the ovary or uterus was extirpated in 1830, by Professor John C. Warren.[7] The patient died of profuse hæmorrhage. Dr. Warren warns others against operating under similar circumstances.

The last ovariotomy performed in America during the period we have been considering, that is, previously to 1842, was by Dr. J. Bellinger in 1835. The tumour, which is hard and fibrous, and of the size of the fist, is preserved in the Museum of the Medical College at Charleston. The patient, a negress, aged about thirty-five at the time of the operation, had given birth seven years previously to a child at full term, and had since had several miscarriages. She was in the enjoyment of excellent health upwards of eleven years after the operation.[1]

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[1] 'Diseases of Women.' New Sydenham Society's edition, p. 114.

[2] 'The History and Statistics of Ovariotomy; a Prize Essay,' by George H. Lyman, M.D. Boston, 1856, p. 87.

[3] 'Boston Medical and Surgical Journal,' vol. xxv, Aug. 25, 1841, p. 41.

[4] Ibid., p. 42.

[5] 'American Journal of Medical Sciences,' vol. xxi, 1837.

[6] 'New York Medical and Physical Journal,' vol. ii, new series, January, 1830, p. 285. 'London Medical Gazette,' vol. v, November 28, 1829, p. 271.

[7] 'Surgical Observations on Tumours,' by J. C. Warren, M.D. Boston, 1837, p. 590.

The author now returns to England and first meets with reports of two operations of gastrotomy, in which, however, there was an error of diagnosis, by Mr. R. C. King, of Saxmundham, Suffolk ('Lancet,' January 21st, 1837, pp. 586 and 587). A successful case by Mr. Jeaffreson and the same Mr. King, operated on by the former on the 8th May, 1836, is recorded in the 'Transactions of the Provincial Medical and Surgical Association,' vol. v, London, 1837,, p. 239, and in the 'Lancet,' loc. cit., p. 588.[2] The mode of operation adopted by Mr. Jeaffreson was Dr. Nathan Smith's, known as the minor operation. On the 12th of the following July, Mr. King operated successfully on the same plan ('Lancet,' loc. cit.), as did Mr. W. J. West of Tunbridge, on the 2nd of November. Mr. West's patient was alive and in perfect health in the year 1853, seventeen years after the operation.[3] Mr. West subsequently operated three times. 1. Minor operation, successful. 2. Same, operation given up on account of adhesions, the patient recovered from its effects and was afterwards tapped seventeen times. 3. The ovarian cyst was removed, and the patient died.

In 1838, the minor operation of ovariotomy was performed by Mr. Crisp, Surgeon, Harleston, Norfolk. The patient lived fifteen years after the operation, and enjoyed particularly good health. She had laboured under ovarian dropsy for twenty years.[4]

In 1839, an attempt at ovariotomy was made in Guy's Hospital, by Mr. Morgan. The operation was given over on account of adhesions, and the patient died forty-eight hours later.[5]

Ovariotomy was performed in the Marylebone Infirmary, by Mr. Phillips, on the 9th September, 1840. The patient died rather more than three days later of choleraic diarrhœa, from which, unknown to Mr. Phillips, she had suffered before the operation.[6]

About this time ovariotomy was attempted by Mr. T. Hargraves. The operation was given over on account of complication with a tumour as large as a child's head, to which the wall of the cyst was adherent. The patient died on the fifth day.[1]

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[1] 'Revue Médicale,' 1847, vol. iii, p. 99. From the 'Southern Journal of Medicine and Pharmacy,' vol. ii, p. 241.

[2] "On the History and Progress of Ovariotomy in Great Britain," by T. Spencer Wells, in the 'Medico-Chirurgical Transactions,' published by the Royal Medical and Surgical Society of London, vol. xlvi. London, 1863, p. 33.

[3] 'Lancet,' Nov. 25, 1837, p. 307, and Dr. Robert Lee, 'Clinical Reports of Ovarian and Uterine Diseases.' London, 1853, p. 90.

[4] 'Lancet,' Nov. 16, 1839, p. 287.

[5] See Mr. Gorham's paper in the 'Lancet,' October, 1839.

[6] 'London Medical Gazette,' Oct. 9, 1840, p. 83.

"With the year 1842 the history of ovariotomy enters upon a new era, for it was in that year that Dr. Charles Clay, of Manchester, commenced the series of operations, which more than anything else has made ovariotomy what it now is, the greatest triumph of modern surgery.

"We have thus far followed the operation step by step, and in detail reviewed each particular case. It would be too tedious, and involve too great loss of time, to pursue the same course longer. The earlier history of ovariotomy in England and America may in a certain sense be regarded as closed; statistics follow."—p. 64.

"It is," observes Dr. Sköldberg, "the still living Dr. Charles Clay of Manchester, whom humanity and science have to thank for the success of M'Dowell's operation.

"From the 10th June, 1842, to December, 1864, Dr. Clay performed 111 completed ovariotomies, with seventy-seven recoveries and thirty-four deaths. * * * His method of operating was the same as M'Dowell's and Lizars'—an incision so large that the tumour might, if possible, be taken out in its integrity. The ligatures around the pedicle were brought out through the lower angle of the incision; the operating room was warmed to between 75° and 80° Fahr., &c. &c. But his success depended not so much upon the method of operating; it had its source in something infinitely more important, namely, in the fact that he did not perform his operations in a large hospital."

The author enters upon a statement and refutation of the several objections which have from time to time been brought against the operation. We have, however, in endeavouring to lay before our readers a complete, though greatly abridged, history of ovariotomy already followed Dr. Sköldberg at such length, that we shall be obliged to pass over this portion of his valuable paper.

"The contest respecting the justifiability of ovariotomy is long since over." Dr. Sköldberg considers that the verdict of the medical and surgical professions in England is all but universally in favour of the operation.

Simultaneously with the dispute regarding the justifiability of ovariotomy arose another controversy: namely, as to the preference to be given to the *major* or *minor* operation.

"Since Spencer Wells," observes the author, "subsequently came forward as the advocate of the minor operation, it has been tolerably generally admitted that the greater must yield the palm to it."

It was at first Dr. Sköldberg's intention to have given a complete report of all ovariotomies performed previously to 1842, and therefore of the German operations during that period, but he soon found the truth of Dutoit's statement, that—

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[1] Dr. Robert Lee, loc. cit., p. 91.

> "The history of ovariotomy in Germany presents, properly speaking, only a series of membra disjecta, so that it is extremely difficult to form any idea of the stages of development of the operation.[1] Of late years (he adds) the operation has, so far as I know, almost entirely died away in Germany. It is certainly sometimes performed here and there, but the result is not published, and we may hunt in vain, through all journals, to find any accurate reports of the same.[2]
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> "France longest withstood this Anglo-American operation, and in the last period, the opinion of the French physicians respecting ovariotomy was quite independent, and was not in the least influenced by the results obtained in England."

In proof of this statement, Dr. Sköldberg quotes from a discussion in the Académie de Médecine in November, 1856.[3]

> "The operation is now, however (he adds), in a fair way to obtain citizenship in French surgery.
>
> "In Sweden ovariotomy has of late years been occasionally performed, and in three cases with a successful result; and as it has not met with any public opposition, it may be anticipated that it will soon be more generally employed." (p. 73.)

The author next passes to the more practical part of his work, but as our principal object has been, as already stated, to give a condensation of his *history* of the operation, we must pass very rapidly over the remainder of the article. The second division of the latter is on the present state of the operation. We have now statistics of more than 1000 operations, and the number is increasing daily. Mr. Spencer Wells, of London, and Dr. Thomas Keith, of Edinburgh, "the world's most successful ovariotomists," have performed 256 concluded ovariotomies. Of these 256 cases a full and very carefully drawn up table is appended to the article, exhibiting at a glance the number, date, age, number of tappings, nature and weight of the tumour, length of the incision, adhesions, treatment of the peduncle, and the result, the mode of death being added, in the fatal cases, in a foot-note. Mr. Spencer Wells' cases, enumerated in the table, are 200, of which 138 ended in recovery, and 62 proved fatal. Dr. Keith's were 56, with 44 recoveries and 12 deaths. Dr. Sköldberg treats the whole subject ably and fully under the several heads of indication, prognosis, preparatory means, performance of the operation, treatment of the peduncle, after treatment, &c., rendering his descriptions still more intelligible by the aid of woodcuts.

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[1] 'Die Ovariotomie in England, Deutschland und Frankreich, von Dr. Eug. Dutoit. Würzburg, 1864, p. 4. [2] Loc. cit., p. 5.

[3] 'Bulletins de l'Académie de Médecine,' 1856—1857. As a sign of the times, it may be mentioned, that this discussion is excluded in the French translation of Sir James Simpson's work on 'Acupressure.'

c. Dr. Oedmansson records a case of cortical encephalitis and acute hydrocephalus, intended as a slight contribution to the study of the functional topography of the brain, if we may so speak, which is now attracting so much attention. "Pathology embarks in the service of physiology to assist in the elucidation of the most important question in the latter science."

The patient was an artilleryman, aged twenty-nine, whose illness commenced on 9th January, 1865, with symptoms at first resembling those of intermittent fever. Cerebral phenomena, however, soon supervened; delirium, loss of memory, stupidity. On the 4th of February it is noted that—

> "Yesterday afternoon he began to be particularly restless, to rave and scream. This state continued all night. To-day he lies on his back, with his body in constant, general, slight tremor. He has a dull look. The pupils are small, but equal and moveable. To questions he gives incoherent and lengthy replies, speaking at first in Swedish, but afterwards in a well articulated, but perfectly incomprehensible gibberish, which he delivers with much declamation."

Some stiffness in the neck when he endeavours to stoop forward. This effort seems to give him pain. Eats greedily what is offered to him. Paralytic symptoms came on on the 28th, the right angle of the mouth being drawn somewhat upwards, while the tongue deviated to the left. The left arm and leg were also paralysed. On the 10th March the right pupil was somewhat larger and less moveable than the left. He did not answer questions, but seemed to be not wholly unconscious. He died on the afternoon of the 11th.

The principal points in the post-mortem examination were as follows:—On the anterior and middle lateral portions of the right hemisphere of the brain the soft membranes projected here and there in the form of cysts as large as nuts, and filled with serum. In a couple of very small spots (of the size of peas) at the anterior part of the fossa of Sylvius, and somewhat anterior to and to the right of the chiasma, they were infiltrated with an inspissated puslike mass. On cutting through the right hemisphere, on a level with the corpus callosum, a spot was met with somewhat behind the middle of the hemisphere, and near the surface of the brain, about two inches long, one inch broad, and three quarters of an inch deep, where the white substance was softened. Corresponding to the focus the cortical substance in the small windings behind the gyrus ascendens posterior was much thinner than is normally the case; it had a yellowish-grey colour, a dry, granular surface of section, and a brittle consistence. In the lower part of the gyrus ascendens posterior, as well as in the whole lower lateral portion of the frontal lobe, to within half an inch or an inch of the summit, the grey substance was throughout more or less altered. Both lateral ventricles were somewhat distended, containing a considerable quantity of a finely flocculent, turbid, greyish-red fluid. In the posterior cornu was a loose, yellowish-green, puslike mass, that on the right side filling the apex of the cornu like a plug. The ependyma over the corpora striata, and septum was studded with fine, shining granulations. The fornix was pale and softened.

D. Professor Key gives a paper on the natural occurrence of *trichinæ*, on a correct solution of which question, he observes, and on the mode in which the pig is infected with them, depend all the prophylactic measures which we can, with any prospect of success, adopt, to prevent the infection of that domestic animal, so important for our sustenance.

It would appear that the trichina spiralis never occurs in birds, fishes, amphibious, or invertebrate animals, and that these cannot even be experimentally infected. Even some swine are immune, which seems to depend on peculiarities of race. The rodentia are in general very easily infected, as are cats and hedgehogs. With the dog a number of experiments have resulted in the development only of intestinal trichinæ, without any invasion of the muscles by the brood taking place. Though, therefore, dogs do not enjoy complete immunity, they are, in general, not easily infected.

The ruminants appear to be scarcely susceptible of trichinous infection. Professor Sjöstedt succeeded, however, in infecting a goat and a sheep. He thinks it important that the infected food should be given in a fluid form, or cut up in small bits in water, so as to avoid rumination, which probably prevents infection. It would seem, in fact, so far as experiments have gone, that all the mammalia are, with more or less difficulty, capable of being infected.

Nevertheless, trichinæ are fortunately not so widely diffused in nature as might be expected from this fact. Many animals are protected from trichinosis not only by the difficulty of infecting them, but also by the nature of their food.

The statements as to trichinæ having been found in vegetables are incorrect.

Trichinæ have been found abundantly in recently captured rats and mice. It is an interesting fact that several of these animals were hardly able to walk, and were easily caught.

In addition to men and swine, the animals which have been found *spontaneously* infected with trichinæ are rats, cats, foxes, polecats, martens, and hedgehogs. There is no doubt, however, that these dangerous parasites occur also in other carnivorous animals, not yet fully examined.

It is quite certain that trichinæ are not conveyed to animals in any kind of vegetable food. Their existence is connected with the animal economy, and only within the muscular filaments do the embryos, which have migrated from the intestinal canal, appear to be capable of attaining their further development. Two modes of this transference have been assumed—1, the consumption by one animal of the trichinised flesh of another; 2, the consumption by one animal of the trichinised excrement of another.

The intestinal trichinæ, however, even when eaten in mass, are usually digested, and do not produce infection. Hence the danger from the latter source is much less than it would otherwise be.

If trichinæ occasionally make their way to exclusively graminivorous animals, such transfers must be very rare. It is from the pig that the danger of the presence, diffusion, and continued existence of trichinæ constantly threaten us; and the author is of opinion that no reasonable doubt can now be entertained that this animal is infected by rats. It is ascertained that pigs eat rats, whether they find them dead, or catch them living; and it is precisely the affected rats which, being more or less lamed, would be more easily caught. The majority of the trichinised pigs examined in Sweden were fed by bakers and millers, about whose premises rats swarm in great numbers. From this and other circumstances the author concludes that the best prophylactic means against the spread of trichinosis, will be (cleanliness, &c., not being neglected), to prevent pigs feeding on the dead bodies of cats, dogs, and other suspected animals, and above all, to take measures for the extirpation of rats, and their exclusion from places where pigs are kept.

E. Hjalmar Lindgren, licentiate in medicine, contributes an elaborate essay of forty-four pages, illustrated with five most beautifully-executed plates, upon the structure of the uterus in the human subject. It would be quite impossible, however, to give an abstract of this valuable paper within reasonable limits.

F. From the report on the instruction given at the Carolinean Medico-Chirurgical Institute during the sessional year 1865-66, it appears that the total number of medical students entered was 122. Synopses of the several courses of instruction, with the number of pupils attending each professor, are added.

2. Dr. Rasmussen, in his valuable paper, shows that extensively as the subject of hæmoptysis, especially in connexion with pulmonary phthisis, has engaged the attention of investigators, the source of these hæmorrhages, the absolute demonstration of the vessel or vessels whence the blood has come, has almost entirely failed.

Of earlier writers, Gilibert has spoken of "varices pulmonum" as the cause of hemoptysis and phthisis; Morgagni, in the dissection of individuals who had died of hæmoptysis, has seen tubercles with dilatation of the surrounding vessels; De Haen observed an aneurysm opening into a cavity; Portal found ruptured vessels from the lymphatic glands opening into the bronchi, and J. Frank, on injecting the pulmonary artery, saw the injected mass penetrate into a cavity.

While the cause of pulmonary hæmorrhages was thus formerly sought in rupture of large vessels, more modern investigators, led by Laennec, have attributed them, where no ruptured vessel was demonstrable, to exudation or capillary hæmorrhage from the bronchial mucous membrane. What chiefly led to this change of opinion was the discovery, first made by Baillie, and subsequently confirmed by Laennec, J. F. Meckel, Schroeder van der Kolk, Guillot, and others, that the vessels in the condensed tissue surrounding pulmonary cavities are obliterated.

Andral enumerates three sources of pulmonary hæmorrhage: 1. From the bronchi. 2. From pulmonary apoplexy. 3. From a cavity. Other writers consider that hæmorrhage in phthisis stands in a more or less direct relation to tubercular deposition. Thus Engelstedt believes that the expectoration of blood depends, not so much on tubercles as on the altered collateral circulation in the inferior, and not yet condensed portions of the lung, whose vessels remain distended with blood, and may, therefore, easily burst when an exciting cause, as a fresh deposition of tubercle, or blow on the chest, &c., occurs. Walshe attributes hæmorrhage in tuberculosis to molecular rupture of the capillaries of the parenchyma, except in the very rare cases where a vessel of more considerable size is ruptured.

Rokitansky looks upon hæmorrhages in the commencement of phthisis as rare and scanty, at a later period as copious and frequent, then proceeding from branches of the pulmonary artery situated in the condensed walls of cavities, especially of bronchiectatic cavities, and in isolated cavities with aborted tubercles. The vessel becomes exposed, partly by fresh deposition of tubercles, partly by a necrotic process in the wall of the cavity, so that it comes to want its accustomed support; it is soaked with the contents of the cavity, becomes thereby macerated, jelly-like and soft, and at last bursts into the cavity. Very frequently this is preceded by an aneurysmatic dilatation of the vessel in towards the cavity.

Hérard and Cornil attribute the expectoration of blood in chronic phthisis to ruptures of small capillary vessels from little fungous excrescences in the walls of recent cavities. In cavities of longer standing the vessels are obliterated in their immediate circumference. In chronic pneumonia hæmoptysis is, according to them, a symptom of the last period (ulceration), while in tuberculosis it occurs in the commencement of the disease.

Dr. Rasmussen's paper is based upon the observation of eleven cases in which the patients died during violent hæmoptysis. These cases fall into two groups: 1. Those where the hæmorrhage proceeded from rupture of a vessel running in the wall of a cavity; and, 2. Those where it was due to rupture of an aneurysm of the aorta into a bronchus or into the pulmonary tissue itself. The first group consists of eight cases, the second of three. Hæmorrhagic infarctions (pulmonary apoplexy), pulmonary gangrene and cancer of the lungs, in the course of which hæmorrhages, though in general not profuse ones, also occur, are not unfrequently met with in the "Kommunehospital," but have never been attended with such considerable hæmoptysis as to cause death, and have consequently not been the subject of anatomical investigation.

(1) *Hæmorrhages from a cavity.*—These are due partly to ruptures of small sac-like aneurysms, varying in size from that of a walnut to that of a pea and under, developed on branches of the pulmonary artery running in the walls of cavities; partly to dilatations (ectasias) of similar vessels with operculated rupture. These two forms occur with equal frequency, each of them namely in four cases.

Ectasias are smaller aneurysmatic dilatations of vessels, running in the walls of cavities. They occur under two forms, first as cords of different lengths on the inside of the cavity, which on being slit up exhibit a slight dilatation of their caliber in towards the latter, with corresponding thickness of the walls. This form is the rarest, and in the author's eight cases was observed only once in combination with a ruptured aneurysm. These ectasias are, however, not absolutely rare, but are rare only in relation to hæmorrhages. In the second or more ordinary form, the vessel comes in contact with the cavity only in a limited locality. Here an oblong prominence, varying in size from that of a pea to that of a bean, and due partly to dilatation of the caliber of the vessel, partly to thickening of the wall of the latter, is developed. The rupture takes place always in a peculiar manner, a V-shaped slit forming in the wall of the vessel, whereby a kind of valve or lid-shaped flap is developed.

Cavities.—The author does not enter into the much-debated question of the relation of miliary tubercle to chronic pulmonary phthisis. In his eight cases tubercles were met with in other organs than the lungs in four instances only—namely: in the intestinal canal in two, in the larynx in one, in the pleura in one. In one other case there were yellow tubercles in the intestine, but without any characteristic miliary form.

In the four cases in which no trace of miliary deposition was found in other organs, in three (the first, third, and seventh cases), even the so-called peribronchitic depositions were wanting, and only one lung was attacked, in two instances the right, and in the third the left. Under such circumstances the affection must be designated as chronic pneumonia with bronchiectatic cavities. The author concludes *that every cavity in the lungs, whose walls are formed by condensed pulmonary tissue, containing non-obliterated vessels, may be the seat of aneurysms or aneurysmatic dilatations with consecutive ruptures.*

Rokitansky looks upon aneurysmatic bulgings in the walls of cavities as due partly to the want of support to the vessel caused by the progressive ulceration; partly to imbibition of the wall of the vessel, which becomes soft, jelly-like, and finally bursts. This imbibition Dr. Rasmussen has not seen.

This defective support of the wall facing the cavity acquires additional value from the fact, that the rest of the vessel lies in a firm, unyielding connective tissue, whereby the intravascular pressure is much increased, and also from the obliteration of so many vessels in the condensed pulmonary tissue, as well as from the small size of the efferent vessel of the aneurysm, which is often speedily lost in the condensed tissue.

A remarkable circumstance in these aneurysms is the absence of coagula, even in those which attain to the size of a walnut.

In support of his views the author refers to three communications in English medical literature, viz., a letter by Dr. Fearn to the editor of the 'Lancet,' 1840-41, vol. i, p. 679; a case by Dr. Cotton of phthisis and fatal hæmorrhage caused by the rupture of a small aneurysm on a branch of the pulmonary artery ('Medical Times and Gazette,' vol. i, 1866, No. 811, for 13th Jan.); and a paper by Dr. Peacock in the 'Edinburgh

Medical Journal,' vol. iii, describing a case brought by him before the Anatomical Society of Edinburgh, where he exhibited in 1843 the right lung of a man aged twenty-nine, who died suddenly after violent hæmoptysis.

Fatal hæmoptysis in a clinical point of view.—Hæmoptysis due to an aneurism or to an ectasia of a branch of the pulmonary artery usually occurs suddenly, and is often fatal on its first invasion. Occasionally, however, it is less violent, and does not so abruptly terminate life, assuming a remittent character. Of the author's eight cases, five belonged to the former and three to the latter class.

As to the frequency of fatal hæmoptysis occurring in this mode, they took place in the Municipal Hospital during the last two years in the following numbers:

In 1866, among 79 dissections of phthisical patients, four times, or 5 per cent.

In 1867, among 104 dissections, once, or 0·9 per cent.

It must be borne in mind, however, that patients do not apply to the hospitals on account of their aneurysms, and it is a mere chance if the rupture takes place during their stay in the house. The sudden death which carries off the majority of these patients, causes them only exceptionally to come upon the dissecting table, as death, even if they seek the hospital, most frequently occurs before they reach it. The author is of opinion that profuse and fatal hæmorrhages always proceed from cavities.

According to the author's views, "pulmonary hæmorrhages, or at least the more violent ones, come to stand in a more remote relation to tuberculosis than has hitherto been admitted; indeed, are related to it properly only so far as chronic pneumonia with bronchiectasis is so frequently connected with tuberculosis. This is also the reason why even copious hæmorrhages in general do not affect patients except by the loss of blood, as chronic pneumonia with bronchiectasis is not, as such, attended with direct danger to life."

Dr. Rasmussen, however, does not mean to say that every parenchymatous pulmonary hæmorrhage is to be explained as proceeding from a cavity. In the more "florid" forms of phthisis such hæmorrhage is certainly due to a breaking up of the infiltration. The slighter hæmorrhages, too, occurring in the commencement of chronic consumption, he would be rather inclined to refer to the perforation of capillary pulmonary vessels by the development of tubercle in the adventitious structures; positive data on this point are, however, entirely wanting.

The practical inference from the author's views is the extreme necessity of endeavouring by suitable means, and best of all by narcotics, to allay the fits of coughing in cases of considerable hæmorrhage from the lungs, so that the thrombosis in the ruptured vessel may have time to form. Digitalis, secale cornutum, acetate of lead, oil of turpentine, &c., Dr. Rasmussen looks upon as probably rather injurious than beneficial.

(2) *Hæmorrhages from ruptured aneurysm of the aorta.*—Of the twelve cases of fatal hæmoptysis, on which the author's essay is based, the source of the hæmorrhage was in three a ruptured aneurysm of the aorta. The rupture occurred once in the ascending aorta and the arch, twice in the descending thoracic aorta. The perforation took place once into the right principal bronchus, once into the left, and once into the left lung.

Hæmoptysis due to a ruptured aneurysm of the aorta is usually profuse, and rapidly causes death. In eighty-three cases of aneurysm of the aorta analysed by Lebert, the rupture took place in fifteen into the bronchi. Violent attacks of hæmoptysis have, however, occurred so long as three weeks before death. *Slight* attacks of hæmoptysis frequently occur during the course of aneurysms of the aorta without any rupture having necessarily taken place into the air passages, as was the case with the late celebrated English surgeon, Mr. Liston. Under such circumstances the attacks of hæmoptysis may be due exclusively to disturbances of the circulation in the lungs, produced by pressure of the aneurysm. Only in one of the author's three cases did the hæmoptysis occur suddenly and produce immediate death, and, in this instance, coagula were completely wanting.

It has been commonly supposed that aneurysms, and especially those of the aorta, do not occur with pulmonary tuberculosis. Lebert, however, in his 83 cases, met with tuberculosis three times. The author describes a case of cavernous pulmonary phthisis combined with aneurysm of the descending thoracic aorta perforating the right bronchus.

Dr. Rasmussen concludes his important essay with some further remarks upon the relation between hæmoptysis and phthisis, specially with reference to the recently published work of Niemeyer.

Until the time of Laennec hæmoptysis was looked upon as one of the most frequent causes, though by no means the sole cause of phthisis. Laennec, on the other hand, attributes the hæmoptysis to the presence of tubercles. Niemeyer reverts to the former opinion, a view the correctness of which is at some length disputed by our author.

3. From H. Steffens' " Report on the Occurrence of Cholera

in Christiania in 1866," it appears that from the 8th of August, on which day the first case of the disease was announced, to the 27th of September, when the last case which occurred terminated fatally, 39 individuals were attacked with distinctly developed Asiatic cholera, of whom 28, or 73·6 per cent., died. Ten cases were entered from eight different vessels in the harbour. The other 29 cases occurred in twenty-one different places in the city, of which some were very wide apart. In fifteen houses only a single case occurred, in two houses 2 cases, and in three houses 3. Of those attacked 29 were males and 10 were females. The author expresses his conviction that the disease was imported into Christiania by a ship captain, who left Clackmannan, in Scotland, on the 18th July, arrived at Christiania on the 2nd August, and took ill the next day, but it does not appear that cholera prevailed at the time at the former place, nor is it shown that there had been any communication between the ship captain in question and any of the seamen subsequently attacked, but merely that their vessels were lying in the same part of the harbour. A clearer case of contagion is shown in the person of a sailor from Hull—which port was then infected with cholera—who suffered from cholerine on the voyage, and on his arrival was not yet quite recovered. During his stay with his family his daughter sickened, and subsequently his mother, and both these persons died. The author, however, admits that "it can scarcely be said that our knowledge of the mode of dissemination of the disease has gained any positive addition by the experience of this year."

Our limits will not allow us to enter into a consideration of the principal portion of Hr. Steffens' excellent report, nor of the reports of Hrr. Lund and Lochmann, that of the latter physician being particularly elaborate. The remaining contents of the volume are two appendices, the first consisting of extracts from the 'English Sanitary Act, 1866,' the second of the 'Report of the International Sanitary Congress of Constantinople on the Course and Mode of Propagation of Cholera in 1865.' The conclusions of the Congress (July, 1866) are—

(1) That cholera is propagated by the intercourse and peregrinations of individuals, by whatever means these may take place.

(2) That the extension of the disease is the more to be feared the more rapid and the more numerous are the means of transit from place to place.

(3) That all other circumstances being equal a great mass of infected human beings, or a single diseased individual, may transfer cholera to great distances.

4. 'The Medical School in Reykjavik' contains two polemical essays, one by District-physician Jón Finsen, on "A Couple of Communications from Dr. Hjaltelin on the Echinococci endemic in Iceland," and one by Dr. H. Krabbe, criticising a review by Dr. Hjaltelin, of Docent Paykulls, "A Summer in Iceland" ("En Sommer i Island"). The communications by Dr. Hjaltelin referred to, are contained in—(1) A thesis by A. A. M. Jacolot, "Relation médicale de la campagne de la Corvette l'Artémise en Islande," 1857. 'Thèse pour le Doctorat en Médecine.' Paris, 1861, pp. 38, 39, the essential part of which is reproduced in the 'Annales d'Hygiène,' 2ème Série, xxvi, 1866, under the title "Contributions à la Géographie et à la Statistique Médicale de l'Islande, d'après des documents plus ou moins récents," by Dr. Beaugrand. The statements of Dr. Hjaltelin, challenged by Dr. Finsen, seem to be three in number, first that the milk of sheep is found to contain "hydatids;" if this were the case, observes Dr. Finsen, echinococci tumours should be met with in the udders of these animals, which is denied. Secondly, it is denied that echinococci cysts are ever seen to be discharged in sheep by suppuration through the abdominal wall (as may sometimes take place in the human subject), and it is added—

> "That, therefore, it is quite inconceivable that Dr. Hjaltelin should have found echinococci-cysts in the field, proceeding from such a rupture in the abdominal wall of the sheep as is mentioned in the French essay."

Thirdly, as to a statement that—

> "By direct experiments on cats and dogs Dr. Hjaltelin has convinced himself that hydatids, which were thus found on the herbs serving as food for sheep, may again produce the hydatid disease in the animals who have consumed them,"

Dr. Finsen remarks that—

> "It is well known that the 'hydatid disease' (formation of echinococci cysts) does not occur in dogs or cats; on the contrary, Siebold showed, long before 1857, that on feeding dogs with echinococci-cysts the Tænia echinococcus is developed."

(2) The author criticises rather severely, and at considerable length, a paper published by Dr. Hjaltelin in the number of the 'Edinburgh Medical Journal' for August, 1867, "On the Treatment now used against the Hydatid Disease in Iceland," pointing out "essential discrepancies and suppressions of important elements in the estimation of the sanatory action of Kamala," in the first two experiments made by him with that

remedy in the disease in question, as shown by a comparison of the essay written in English with the same cases published by Dr. Hjaltelin in an Icelandic Journal, 'Thjódólfur," for 1864, "Om Echinokokkerne," pp. 154, *et seq.* Dr. Finsen takes the opportunity, also, to reply to "an attack by Dr. Hjaltelin upon Récamier's method" and himself. An abstract of Dr. Finsen's remarks upon this operation will be found in the number of this Review for April, 1868, p. 348.

(3) To give a notice of Dr. H. Krabbe's critique on Dr. Hjaltelin's review of Docent Paykull's "Summer in Iceland," would occupy too much space in the present article.

5. Dr. Stadfeldt's case of lumbo-sacral cyphosis was that of a married woman, æt. 42, pregnant for the third time, admitted into the Lying-in-Hospital in Copenhagen, 1st June, 1866. In her second year she had fallen down stairs and injured her back, an accident which resulted in the development of cypho-scoliosis in the lumbar region. At the termination of her first pregnancy she was delivered in the institution by perforation and cephalotripsy. Her second delivery took place three years later: it also was instrumental; the labour was difficult, and the child stillborn. On the third occasion, the forceps having been first tried at 7 a.m. on 2nd June, and proving ineffectual, perforation and cephalotripsy were again resorted to. The child was a boy at full term, five and one eighth pounds in weight, without the blood and cerebral substance lost, and nineteen and a quarter inches in length. Peritonitis soon set in, and proved fatal on 4th June.

On post-mortem examination, traces of caries, with destruction of the bodies of all the lumbar vertebræ, were met with, and in the place of the latter was a bony uneven mass, whose height anteriorly was scarcely one centimètre on the right and scarcely two centimètres on the left. By reason of this destruction the spinal column had been split, so that it deviated anteriorly and to the right with the part lying over the destroyed bodies of the vertebræ, but there was at the same time a projection of the column over towards the right side, causing the newly formed uneven bony mass to project beyond the left margin of the base of the column to a breadth of about one and a half centimètres. On the whole the lumbar portion of the spinal column seen from behind formed a sharp curve, with the concavity forwards, and a little to the left.

The pelvis exhibited no diminution in its upper aperture, but the space diminished gradually downwards, the ossa ischii being pressed towards one another, and the lower extremity of the sacrum and os coccygis being pushed forwards, reducing the conjugate diameter in the inferior opening, in the prepared pelvis (from the point of the sacrum) to about 3½″ (from the point of the os coccygis to 2¾″), and the transverse diameter to 2″ 10‴.

So few accurate descriptions exist of cyphotic pelves, that the author feels it his duty to record as correctly as possible every case of this abnormity which occurs to him. He reproduces in a tabular form a large number of elaborate measurements by Prof. Breisky of Berne, who first made the cyphotic pelvis the subject of accurate measurements.[1] From a comparison with the normal pelvis it would appear that there was in Dr. Stadfeldt's case a diminution of distance between the spinæ ischii of 26·6 mm., and between the tubera ischii of 39·9 mm. He proceeds to point out some abnormities in which the pelvis described by him deviates from the law laid down by Breisky, especially with reference to the relation of the os sacrum and the signification of the position of this bone with respect to the conjugate diameter in the inferior pelvic opening; but these deviations depend evidently on the seat of the cyphosis down at the os sacrum.

It should be mentioned that, after her first delivery in the hospital, the patient had been enjoined in future to present herself at an early period of pregnancy, that artificial premature delivery might be adopted. This, however, she neglected to do.

6. From Dr. Stadfeldt's 'Report of the Royal Lying-in Institution in Copenhagen,' it appears that 1255 women were admitted into the hospital and its branches during the year. The total number of women delivered was 1237. Of these, 22 gave birth to twins, 859 were unmarried, widows, or separated from their husbands, 378 were married, 585 were primiparæ, 271 births were premature, 988 were at full term. Of the children, 620 were boys and 636 were girls, the latter preponderating in the proportion of 100 : 97, being about the reverse of the usual state of things: 1169 children were born alive, and 90 were stillborn: 511 of the women were treated in the institution itself, and 744 in the branches.

Narrowness of the pelvis was met with 17 times. The narrowest diameter varied between 2¾″ and 3½″. In these cases delivery terminated 8 times naturally, twice by turning and extraction, once with the forceps on the head coming last, once in a case of foot presentation, with extraction of the feet and the forceps on the head; three times with the forceps on the presenting head, and three times by cephalotripsy. Premature delivery was twice artificially induced; in one instance a living child was thus naturally born.

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[1] 'Medic. Jahrbücher,' Wien, 1865, 1ste Hefte.

Hydramnios to a great degree occurred once in November. There were twelve or thirteen quarts of liquor amnii. The child was a girl born nine or ten weeks before its time, malformed with anencephalia and spina bifida.

The forceps was applied thirty-two times, or once in thirty-nine deliveries. Cephalotripsy was performed three times, turning eight times.

The puerperal state was during the year on the whole more unfavorable than in the preceding one, which was due partly to a slight epidemic of puerperal fever in April and May, partly to the frequent occurrence of complications during pregnancy, delivery, and the subsequent confinement.

The *puerperal* mortality was 1 in 35, or 2·8 per cent. The *total* mortality was 41 or 1 in 30, or 3·3 per cent.

Twenty-six children died during the first twenty-four hours after birth. In addition, forty-five died during the stay of the mothers in the hospital. Of malformations there were 9 cases, viz., encephalocele 1, anencephalia 1, hernia funiculi umbil. 1, harelip 1, closing of the œsophagus 1, varus 1, supernumerary fingers 2, absence of hand and forearm 1.

7. The questions to which Dr. Stadfeldt directs his attention in the pamphlet above quoted are—1. Whether a woman has given birth to a child; and 2. How long since has delivery taken place. He does not enter fully into the question, confining himself to those signs which he believes to require revision, and to those relating to a living individual.

Casper states that the frenulum at the posterior commissure of the vulva may remain uninjured in abortion, but not in ordinary delivery. Montgomery, on the contrary, gives the case of a woman who had borne five children, and in whom, nevertheless, the frenulum remained uninjured. Dr. Stadfeldt, during the last two years, found it intact in five cases among 500 deliveries, or in 1 per cent.

The three points on which the author depends in determining the time which has elapsed since delivery when this exceeds one or two weeks, are the involution of the uterus, the state of the lochia, and the transition from the recent to the fully cicatrised, striated vitiligines in the rete Malpighii.

The physiological involution of the uterus after birth induces, as is well known, an extraordinary diminution of the organ both in length, breadth, and thickness, until it regains, two or three months later, its normal size, form, and structure.

Dr. Stadfeldt has adopted a mode, suggested to him by a statement made by Sir James Simpson in 1843, of determining the state of involution of the uterus. The method consists in measuring the uterine cavity. Sir James Simpson's statement was, that in a couple of cases he was able, with the aid of the uterine sound, to give legal evidence as to births having occurred one or two weeks before the investigations. "It is remarkable," observes Dr. Stadfeldt, "that neither Simpson, nor, so far as I know, any one else, has followed this track." He proceeds to show that the cause of this is probably the fear of the use of the sound giving rise to injury, and we must confess that it would be difficult indeed to persuade us that serious damage would not be likely to follow such a practice. His evidence is, however, strongly in the other direction:

> "Such a fear (he says) I believe to be unfounded, and I cannot assent to Huguier's opinion when he dissuades from the employment of the sound in the puerperal uterus. I have performed such sounding about one hundred times without seeing any injurious result from it, and I believe that in the uncomplicated puerperal state we may, without the slightest risk, undertake the sounding of the uterine cavity, provided one or two weeks have elapsed since delivery; indeed, I may add, that in my opinion the medical jurist *ought*, under such circumstances, to use the uterine sound, because, as we shall soon see, it is capable of yielding him no slight service in the diagnosis of so dangerous a crime as concealment of birth."

The author adds certain precautions to be observed, which, as we have no wish or expectation of seeing the practice adopted in this country, we do not think it necessary to reproduce.

The author's positions as accoucheur-in-chief to the Lying-in Institution, and first physician to the Foundling Hospital or Orphan Asylum (Pleiebörnsstiftelsen), where many of the unmarried puerperal women remain often until six weeks after delivery, have given him unusual opportunities for this special investigation. He uses Sir James Simpson's uterine sound of flexible metal with a broad knob at the point, and has never attempted sounding before the fourteenth day after delivery.

Dr. Stadfeldt gives, in a tabular form, the results of 62 measurements of the "average length of the whole cavity of the uterus." This he found to be, after the lapse of two weeks, in 26 measurements, 3·6″; of three weeks, in 19 measurements, 3·2″; of four weeks, in 12 measurements, 3·1″; of five weeks, in 6 measurements, 3·09″; of six weeks, in 1 measurement, 3″; of nine weeks, in 2 measurements, 2·75″. Consequently even at the end of nine weeks, the average length of the cavity had not fallen to the normal standard of 2·5″. Dr. Stadfeldt at first attempted to distinguish between primiparæ and multiparæ, but he found no marked difference between them.

Dr. Stadfeldt gives also a table of successive measurements from week to week in the same women. The number of patients experimented on in this way was 14, and the results varied considerably. Thus in one case the length had fallen from 3¾″ at the end of two weeks, to 2¾″ at the end of three. In another it seems to have remained stationary at 3½″ at the end of two, three and four weeks respectively.

Dr. Stadfeldt remarks that this method is available at a comparatively late stage, when the other signs of delivery have either disappeared or become little marked. The results obtained in legal investigations would probably also be more decided than those obtained by him, as those women who become the subjects of such inquiries have usually destroyed their children, or at all events have not given suck, under which circumstances it is well known that the involution of the uterus proceeds more slowly.

He does not mean to say that his measurements are an accurate expression of the length of the uterine cavity in the different weeks, for this there is too great variety in different individuals. He wishes to convey merely that it is possible for the medical jurist to trace the successive diminution of the organ in the same person, and thence to deduce his legal conclusion.

After some remarks upon the indications derivable from the lochia and the striated cicatrices arising during pregnancy, the author concludes his essay by pointing out the pathological conditions which might have the same results as pregnancy and delivery, and prove a source of fallacy in connection with his method. These are sufficiently obvious, and do not require enumeration here. We have already expressed our doubt that Dr. Stadfeldt's proposal will meet with a favorable reception in this country.

8. The learned Professor of Midwifery in the University of Norway, undertakes a task for which his long experience, varied acquirements, and personal knowledge, from repeated travel, of most of the great cities and towns of Europe, particularly fit him; an investigation into the hygienic condition of many of the obstetrical institutions on this continent and in Great Britain. The inquiry, too, bears upon one subject which has attracted much attention of late years, viz., as to whether the ends of benevolence and science will be best promoted by the establishment of large lying-in hospitals, of small lying-in hos-

pitals, or by an organised attendance upon parturient women in their own homes.

With such an object, Professor Faye, in 1849, visited Paris, many towns in Belgium and Holland, and London and Copenhagen. In the period which has since elapsed, 1849 to 1867:

> "I have visited," he says "the institutions in Leipzig, Zürich, Bern, Venice, Pavia, Helsingfors, and Petersburgh, with many newly erected lying-in institutions in Würtemburg, Munich, Stockholm, and Copenhagen, and have thus had an opportunity of making myself acquainted with the supposed improvements which have been made with a view to obtain more favorable results from the care of the patients."

Professor Faye has lately also, with the same object in view, made a still more extended tour, visiting Hamburgh, Berlin, Prague, Vienna, and Gratz, thence he proceeded by Trieste and Ancona to Rome, Naples, and Turin; over Mont Cenis to Savoy and Switzerland, and, subsequently, from Bern, to Paris and London.

Interesting as it would be to follow Professor Faye through the practical details into which he so ably enters in his report, we must, on the present occasion, confine ourselves to endeavouring briefly to obtain his principal results, chiefly with a view to a comparison of the several institutions which he visited, and to recording any fact which may appear to be of more prominent importance in connection with the sanitary condition of the same.

The first clinical institution visited was that in Dorotheen Strasse in connection with the University of Berlin. In this there is no special system of ventilation, the latter being effected only by opening windows, and in winter in addition, by the draughts of the stoves. In the beds straw mattresses alone are used, which are emptied after each patient, the tick being washed. There has been no epidemic or endemic puerperal fever for many years, an exemption attributed by Professor Martin to the strict order and cleanliness observed. Each student is allowed to examine only one woman in turn, thus sparing the patients and lessening the risk of contagion.

In the Lying-in Institution in Dresden, under Professor Grenser, the average mortality has not exceeded from 2 to 3 per cent.

The Lying-in Hospital in Prague was formerly a monastery; the building is, therefore, not very well adapted to its present purpose. Professor Seyfert has the direction of the clinique for students, into which three-fifths of the women are received, while Professor Streng is at the head of the division for midwives, to which two-fifths of the women are allotted. The average yearly number of admissions is 3000, some women being, however, received in an adjoining hired house, in which the rooms are so small and the beds so close to one another, that Professor Faye was surprised that serious and endemic disease should not be the result. He was, nevertheless, assured, that the sanitary state of the institution was good, care being taken to keep up powerful ventilation and great cleanliness. The ventilation is accomplished only by windows, which are kept open also in the winter, while the building is thoroughly heated with stoves.

In the great General Hospital in Vienna are three sections for parturient women, one under Professor Carl Braun, which serves as a clinical school for the university students; another, where the midwives are instructed under Professor Späth; and a third for pay-patients. The yearly number of deliveries in all three divisions taken together is 7000. When Professor Faye was attending the first division in 1838, the mortality was fearful, but he was now informed that it scarcely exceeded 1 per cent. This great improvement in the sanitary state of the institution was attributed chiefly to a new and effectual mode of ventilation, and also in part to a regulation thst students attending the dissecting room, should not be permitted to make any examination in the lying-in institution, without first washing their hands thoroughly in a solution of chloride of lime. The system of ventilation referred to seems to consist simply in the use of a hot-air apparatus on a very large scale, and it was admitted that it was properly effective only in winter, and that in summer recourse must still be had to windows and other openings.

> "The engineer Böhm, who has constructed the whole, is considered to have successfully solved his problem, and I entertain" observes Professor Faye, "no doubt, from the experience I have had in our own institution of the efficacy of the same method on a small scale, that it quite answers the purpose. But I do not believe that this arrangement is the sole cause of the remarkably improved sanitary condition, of the reality of which I was moreover convinced. Professor Späth has, in fact, proved by reliable statistics that the sanitary condition of the institution has for many years been good. I must, therefore, give up my doubt, and believe in the possibility of maintaining a healthy condition even in lying-in institutions which, as in the case of that in Vienna, are so large that a similar one will, for other reasons, scarcely be built in any country."—p. 23.

The author was favorably impressed with the lying-in institution in the university town of Gratz, which belongs to the class of smaller hospitals, but as he was not so fortunate as to meet Professor Helly, the physician, he does not enter into details.

Professor Faye next reaches Rome and writes:

> "The Eternal City, as by reason of its numerous monuments of the greatness of past times, it is constantly and correctly called, is, moreover, the seat of an university, which also contains within it a medical faculty and a medical school. Through long ages comparatively little has been heard of the action of this university, and according to what a German colleague, Dr. Erhardt, who has for many years practised as a physician at Rome told me, the cause of this is evidently to be found in the influence which the priestly power directly and indirectly exercises upon free investigation, which is, moreover, not less felt in the field of natural science. Rome awaits the time when it shall be vouchsafed to her sons, unimpeded by the restraint of the mighty popedom, to develope the same living activity, of which we now see the most promising signs in the rest of Italy. The Romans have not degenerated (said Dr. Erhardt), from what their forefathers were; they are possessed of natural endowments, of a lively capability of description and of a firm will, and wait only for freedom to exhibit themselves as the cultivators of science, in a spirit and a direction which will place them on a level with the investigators of the rest of Europe, and we may also predict that in a not very distant future Italy will maintain her ancient fame as a true home of science and of all free inquiry."—p. 30.

The "Instituto Ostetrico della Rocca," under the care of Dr. Panunci, admits about 250 lying-in women yearly.

> "The largest ward contains seven or eight beds, is well ventilated in the most natural mode, having windows at both sides and also a pair at the one end, so as to give a thorough current of air. With this lying-in institution a school for midwives is connected, but no male student is admitted, as the spiritual direction does not consider that this would be correct. I asked Dr. Panunci, who is professor in the university, where the medical students received practical instruction? to which he replied that none such was given, and that he was restricted to theoretical teaching."

The obstetrical arrangements in Naples also appear to be on a small scale. The author visited with Professor Schrön the "Maria e Jesu" Hospital, lately converted from having been a monastery, into a clinical hospital containing four divisions, a medical, a surgical, an ophthalmological, and an obstetrical. The latter consists of four apartments with one or two beds in each, and a larger ward containing twenty beds. Among the benevolent institutions of Naples is a large hospital for incurables, in which not less than 1500 chronic patients and a number of poor parturient women are attended; moreover, there is an asylum for foundlings, in which a number of nurses are employed. "They live, I was told, chiefly upon macaroni and other vegetables, and, accordingly, yield abundant milk."

The Maternity at Turin also is established in an old monastery; it now consists of two services, one for the instruction of the university students, the other in connection with the school for midwives. Both divisions receive about 700 or 800 women yearly, of whom the greater part come in at different periods of pregnancy. No artificial ventilation is employed; and as the wards have windows only on one side, and there is no other communication with the open air, the sanitary state of the house is not the best. The mortality for the last ten years has, in fact, amounted to 4 per cent. in the division allotted to students, which is under the care of Professor Tiboni. In the division for midwives under Professor Rossi, of which no statistical report is published, the mortality is said to have been still greater.

The Lying-in Institution in Zurich is an old and inadequate hospital. Professor Breslau had, however, before a rapid disease unexpectedly carried him off, succeeded in getting the Canton to vote the means for the erection of a new institution on the same elevation, where the new clinical hospital was built some years ago. The hospital in Bern, too, is very unsuitable, but there also a new one is about to be erected.

The hospitals in Paris specially arranged as lying-in institutions are the "Hospice de la Maternité ou d'accouchement," and the clinico-obstetrical service in the "Hôpital des Cliniques." In addition, parturient women from the different arrondissements of the city are received in the larger hospitals, where one or more wards are adapted to this purpose.

The number at present admitted yearly to the Maternité amounts to 4000, and under the existing hygienic conditions, the occasional occurrence of epidemics is unavoidable. Referring to the controversy as to the comparative advantage of lying-in hospitals, and of attendance at the patient's home, the author says:

> "A too predominant and, indeed, exclusive part, has been assigned to the contagious or infectious influence in obstetrical institutions, nor has it been sufficiently put forward, that the wretched women admitted into the public lying-in hospitals very often bear the germs of disease in themselves in a higher degree, than is the case with women who have a fixed home.
>
> "In a communication both of Dr. Le Fort's, and from the statistical works of another well known Parisian physician, Dr. Hervieux, Professor Guyon ('Archives Générales de Médecine,' Avril, 1866), remarks, that too great weight has been attached to the contagious

influence, and in the course of the discussion which took place in 1866 in the 'Societé de Chirurgie,' on puerperal fever, he attributes an essential part to both self-infection and an epidemic constitution, and dwells less on the influence of any contagion.

"It is mentioned by way of example, that in the great Lying-in Hospital in Dublin, the rate of mortality in which has always been regarded as favorable, the sick puerperal women lie together with the healthy until the agony sets in, without endemic disease being thus developed, a result which can scarcely be explained in any other mode, than that the ventilation must be so good, and the rooms so spacious, that no ward infection can take place."

From the reports laid monthly before the Imperial Surgical Society it would appear that the sanitary condition of the Parisian lying-in institutions has for a long time been more satisfactory than formerly.

"If this be really so, it is undoubtedly the result in this case also, of the greater attention now exercised with respect to general hygienic conditions; for it will be observed, that no school is connected with these institutions, and that therefore all the inconveniences attending such an arrangement are entirely avoided, as the physician forbids all unnecessary examinations."

It was stated that the sanitary condition of the obstetrical department in the Hôpital des Cliniques, where the mortality in the years 1835—1848 was 4 per cent., is now good. It would appear, however, that the institution was, in consequence of the invasions of violent epidemics, during the above period closed five times, and the instruction given there suspended, "no slight inconvenience for the medical school in Paris." Professor Faye points out the obvious difficulty which such occasional closing of some obstetrical institutions throws in the way of deducing the comparative mortality of all.

"As the result of all that has been done in Paris respecting the hospital care of lying-in women, it would seem necessarily to follow that the Maternité will some day be replaced by smaller and more suitably arranged institutions, although with the experience of Vienna before our eyes, the opinion can scarcely be expressed so decidedly as formerly, that a very large institution is, as such, necessarily unhealthy." (P. 54).

The author refers for an account of the lying-in institutions of London to his report published in the year 1849, and confines himself in his present work to the estimation of the hygienic conditions, which most nearly concern the case of puerperal women. From Dr. Barnes's 'Statistics' for 1851—60 published in the 'Lancet,' Nos. 6 and 7, 1865, he quotes the childbed mortality in 863,703 births as 1 : 204. From a report published by himself and Hr. Schönberg of the obstetrical mortality of Norway for the five years 1859—63 it would appear that the proportion in that country is one death in 135 confinements. Thus Norway (town and country population together) "stands worse on paper than the city of London." Nevertheless Prof. Faye adds:

> "I have no doubt that the puerperal mortality is in reality more favorable for all Norway than for London, where every kind of misery is met with side by side with every kind of luxury and refinement in society, and the explanation is easily found in the circumstance, that we reckon in a different mode or make up our statistics on a different basis."

The difficulty in instituting a comparison between the childbed mortality in different countries consists, in fact, in that of distinguishing between those affections which stand in a direct causal relation to childbirth, and those which do not. The committee which undertook the investigation of such mortality in Norway, feeling this difficulty, followed the direction first indicated by Sir James Simpson, and proceeded to ascertain how many women died in the first four weeks after delivery, the so-called puerperal month. Calculating that by following this method one half per cent. of not properly puerperal cases would be included in the mortality, we may be pretty sure that about as many actually puerperal affections would end fatally after the lapse of the first month, so that one error would balance the other. Were some such plan adopted in all countries, international comparative statistics would become possible, "and we should then not have so many astounding reports of the favorable mortality in many places in contrast to the very unfavorable state of things in others."

It has been observed both in London and Christiania, that a much larger proportion of deaths in childbirth occur in the higher than in the labouring classes of society. This is attributed by Professor Faye to the greater power of resistance to disease enjoyed by the hardier females of the latter, and that in the former "the capability of nourishing their young and resisting the dangers of the puerperal state is weakened" by their often anæmic condition.

> "In England and Scotland the personal conveyance of infection, and infection in lying-in institutions have always been looked upon as the most essential causes of puerperal fever, while to the latest time the importance of the so-called self-infection through the secretions of the patient herself seems not to have been properly understood."

Most physicians of experience will certainly hold that infection of the system of a puerperal woman may be effected by the personal conveyance of a morbid matter, whether that be of an organic or inorganic nature; but from admitting this as a reality, to assume it as the principal cause of disease, is to make a great jump over many other presumable morbid influences. At the same time there can be no doubt of the great importance of constant personal cleanliness, washing and disinfection of the hands, change of clothes, &c. Sir James Simpson, who certainly decidedly maintains the possibility of the transference of injurious matters by accoucheurs, told the author that he had never conveyed infection since he began to clean his fingers with cyanide of potassium.

Though we have thus dwelt at considerable length upon some portions of Professor Faye's very interesting and valuable pamphlet, we have been compelled to pass over many points of practical importance of which he treats. The learned professor has certainly employed a summer tour to the great advantage of his professional brethren and of his fellow-man, in collecting, within eighty-four octavo pages, so large an amount of valuable information.

One important point clearly established by his extensive observation is, the vast effect of adequate ventilation, of due attention to cleanliness, of the use of disinfectants, and of the avoidance of unnecessarily frequent examinations of the patients, in diminishing the mortality of large lying-in hospitals.

9, 10, 11, 12. This article has already so far exceeded the limits originally intended, that we are compelled to content ourselves for the present with recording the titles of the four remaining works on our list as a contribution to bibliography. Hr. Holmgren's physiological investigations respecting the stomachs of the pigeon, extending to nearly fifty pages, will prove interesting to both the physiologist and the natural historian. This is followed by a paper, reprinted from the 'Upsala Medical Journal,' on the influence of diet on the stomach, illustrated with a plate representing the stomach of an ordinary corneating pigeon, and the changes produced in the stomachs of pigeons by an exclusively animal diet continued through four and six months respectively. Dr. Odenius, already favorably known to English readers, contributes two important papers on minute anatomy, one of which, illustrated with a beautiful plate, after a drawing by himself, has had the advantage of appearing in the German language.

Since the above was written we have received an appendix to the third volume of the "Swedish Archives of Medicine," containing a long paper by Dr. John Bjorkén, on "The Treatment of Urethritis," and "Researches respecting the Vitreous Humour," by C. A. Blix. In the appendix it is announced that the publication of the 'Medicinskt Archiv' will henceforward be discontinued, to make room for a new journal to be called the 'Nordiskt Medicinskt Archiv,' or 'Northern Archives of Medicine,' which is intended to contain original papers in the Swedish, Norwegian, and Danish languages, as well as reports of all original essays in these languages connected with medical science, which do not enter into the journal, but may be of general interest. The new journal, established for the purpose of making the medical literature of all Scandinavia easily accessible both at home and abroad, will be published in Stockholm under the direction of an editor in chief, aided by special committees in Copenhagen, Christiania, Helsingfors, Upsala, Lund, and Stockholm. The first part will appear in 1869.

REVIEW II.

The Morbid States of the Stomach and Duodenum, and their Relations to the Diseases of other Organs. By SAMUEL FENWICK, M.D. London, 1868. 8vo, pp. 388.

DR. FENWICK's design in this volume is not to show the relations of the digestive organs in the production of disease, but the effect which general or distinct diseases have in producing morbid states of those parts. To the furtherance of this end the materials are very large: for, in point of fact, there is hardly any autopsy from which information may not be gained elucidatory of the points at issue. The principal difficulty lies in the post-mortem changes which, in such a perishable tissue as the mucous membrane, are apt to mass into a common aspect many conditions very different during life. For this reason apparently Dr. Fenwick has, in the later examinations herein recorded, paid attention to the blood-vessels, which are much less affected by decomposition, and much more fairly represent the proportionate vital changes. We trust that in future investigations he will continue this course, for there is a wide opening for observation in that direction.

The conditions of the stomach which this author identifies by post-mortem examination are: (1), Acute sthenic inflammation, or gastritis; (2), Catarrh, or subacute gastritis; (3), Degeneration, or chronic gastritis; (4), The desquamative condition.

The first named is extremely rare, so much so that the only instances the author has been able to collect are, one arising from scarlatina, extracted from Dr. Beale's 'The Microscope in Medicine,' one complicated with degenerated kidneys, one (which we should have called catarrh) in a cardiac patient, and one (where the recovery of the sufferer rendered the diagnosis doubtful) in a gouty subject. While, however, acute gastritis is so rare and almost always fatal, chronic gastritis is so common that " in the bodies of few persons above the age of forty, who have died in our London hospitals, can the mucous membrane at this part be said to be in a perfectly healthy condition " (Fenwick, p. 129). It is to this, then, that Dr. Fenwick is disposed to refer the great majority of the examples of " indigestion," " weak digestion," " dyspepsia," which encumber our clienteles.

Between the two lies subacute gastritis, a condition analogous to the " cold in the head," " influenza cough," or whatever name may be chosen to represent a temporary catarrhal state of the respiratory tract. In the stomach it usually produces what the patient calls a " bilious attack." Closely related to subacute gastritis is the condition of the stomach in certain forms of fever; along with the mucus the already formed epethelium is shed, either in separate scales or in patches, just as happens to the skin in the same fevers. Thus the gastric juice is impeded in its excretion from the cells, and prevented from getting to the swallowed food, which consequently lies undigested. There is not, however, any deficiency in pepsin, which is found in nearly the average quantity in the gastric membrane after death; nor in cell-growth, for the mucous membrane is thicker and heavier than natural. This quite accounts for the sudden loss of appetite, nausea, and vomiting, on the invasion of scarlatina and of measles, and for the sndden renewal of power to take aliment immediately the desquamation commences, in proportion to the earliness of that commencement. In direct contrast to this condition is that found in typhoïd fever, which is characterised by the mucous membrane of the stomach being reduced in weight more than one half, and by deficient function in the gastric juice. Hence an equally complete loss of digestive power, and a more gradual restoration. Not but what, according to Dr. Fenwick, an "elimination" of morbid poison takes place into the intestinal canal in typhoïd as well as in scarlet fever; only that its point is the lower part of the ilia, and not as a rule the stomach.

Dr. Fenwick's theory of fever is that a soluble poison has been introduced into the blood, which nature, with the design of diminishing the dangerous effects of its remaining in the circulation, endeavours to get rid of by ruling its union with some solidifying constituent of the vital fluid. In this way "the poison is gradually removed from the system along with the coagulated albumen with which it is deposited. This is chiefly effected by ulceration, or the analogous process of desquamation." We must say this is rather a blundering proceeding to ascribe to a beneficent power; and we feel we ought to modify the advice we give our pupils to "imitate nature" in their treatment. What can be worse than ulcers in the ilia? And is not the normal process of digestion, by which foreign soluble matters in the blood are daily got rid of, the most happy way of disposing of an organic poison?

In cardiac disease Dr. Fenwick finds the gastric membrane in a state of venous congestion, thick and heavy, covered with mucus, and often having torulæ and sarcinæ in that mucus. This accounts for the weight and oppression after food, with distension and flatulence, which are so familiar to us in the early stages of disordered heart, and which later are often only half masked by the more urgent symptoms.

In diabetes, contrary to what one would have expected from the strong appetite and copious ingestion of food, there is no increase found in the solvent glands of the stomach; indeed, the membrane seems to be equally atrophied with the rest of the body. The part of the digestive apparatus most constantly found affected, by the united dissections of many independent recorders, seems to be the pancreas. Dr. Fenwick had noticed this before he was made aware of the observations of others agreeing with his.

Bright's disease of the kidneys, when it destroys the life at an early period, is usually found associated with acute affections of the gastric tubes; while granular kidney, and the other chronic forms of the complaint are characterised by inter-tubular or degenerative gastritis.

In the tubercular diathesis the condition of the gastric mucous membrane is of unusual interest, not only because of the great prevalence of the disease and the clinical importance of the digestion in its treatment, but because two of our most trustworthy observers have come to diametrically opposite conclusions on the matter. Dr. Handfield Jones concludes, as the result of his examinations, "that scrofulous disease, using the term in its widest sense, does not exert any marked influence in the production of organic disease of the gastric gland-tissue." On the other hand, Dr. Wilson Fox states:—"The disease that leads to the evolution of gastric catarrh with peculiar frequency is tuberculosis." Dr. Fenwick applies himself to reconcile this apparent discrepancy. He says:—"Dr. Jones has not noticed the minuter changes of the gastric epithelium, but chiefly directed his attention to the general condition of the tubes; while Dr. Fox more carefully examined those alterations of the secreting cells which he believed to indicate the effects of inflammatory action. The former, therefore, considered tubercular disease as seldom connected with gastric disorder, because he could generally detect the secreting tubes; whilst the latter came to an opposite conclusion, inasmuch as he found the contents of the tubes in an abnormal condition. My own observations confirm the statements of both."

Clinical experience, unhappily open to all from its frequency, will incline us all to agree. He must be a very blindfolded practitioner who does not recognise the impairment of digestion premonitory of consumption, and who is not prepared to have this accounted for by the condition of the gastric membrane in such cases.

The chapter "On the Connection between the Affections of the Brain and the Digestive Organs" reminds us of the oft-quoted one on the snakes of Iceland. There seems, by Dr. Fenwick's observations, to be *no* connection between affections of the brain and degenerations of the digestive organs; but he treats of nervous or exhaustive dyspepsia, which might lead the incautious reader to class that among cerebral disorders, or to suppose that the author does so.

Chapters follow on "Dilatation of the Stomach, Cancer, and Ulceration," which call for no special remark, except that, like the rest of the volume, they are encouragingly supplementary to the many clinical treatises on this subject in the hands of medical readers.

Dr. Fenwick has not, however, confined his attention solely to morbid anatomy. Besides current deductions as to treatment included in the chapters we have been reviewing, he has added an appendix containing a statistical inquiry into several of the efficient causes of indigestion. He clearly shows that, while increased age increases the chances of the patient thus suffering, yet the most usual time of its first invasion is that of completed growth, viz., from twenty to thirty. He might have added that social as well as physiological reasons exist for this. He demonstrates a very decided influence of hereditary transmission, especially from the maternal side in the production of this, as of other disorders. He finds that mental anxiety in males increases by about 7 per cent. those suffering from weak digestion, but diminishes by about 9 per cent. those affected by painful dyspepsia. In females this conclusion is different. (The numbers seem to us rather too small for this distinction to be made.) Contrary to the popular prejudice, but strictly in accordance with modern physiological pathology, gross and plethoric habits are less predisposed to indigestion than the spare. When they do suffer it is not from weak digestion, or painful digestion, but acidity. To that, however, they are less liable than the spare. The effects of imperfect mastication on the digestion are not so obvious from collected evidence as might have been expected. Perhaps the difficulty of defining a satisfactory test of imperfection may account for this. It is remarkable, though, that among the typical instances of causes of indigestion given in Dr. Chambers' work on the subject, imperfect mastication does not figure. As regards the use of tobacco, again, Dr. Fenwick's investigations support the opinion of Dr. Chambers in rejecting the "counterblasts" which have been hurled against smoking. It appears rather to benefit than injure the digestive forces. The question of snuffing is not entered into in the volume at present under review.

As regards the use and abuse of alcoholic stimulants, it would appear that while excess disposes to dyspepsia, yet total abstinence does so still more notably. The healthiest in this respect were the moderate consumers. The most surprising fact in these figures is that the proportion is greater in favour of the men taking both ale and spirits than of those using malt liquors only. Dr. Fenwick, however, is cautious against being set down as a bigoted publican's advocate, for he wisely remarks that when a person, from a natural mental defect or other causes, is liable to be led into excess, it is better to suffer any amount of dyspepsia than to subject himself to the innumerable evils resulting from drunkenness. The number is small from which conclusions are drawn respecting the influence of deficiency of animal food, but it seems especially injurious in the middle periods of life. The injurious effects of the excessive use of tea—that is to say, tea taken more than twice a day, and still more of coffee, are clearly shown. As to outdoor exercise, it appears that of healthy persons examined 75 per cent., while of dyspeptic persons only 50 per cent., enjoyed this advantage. We would especially recommend for imitation Dr. Fenwick's ingenuity in working up into experience the waste material for useful knowledge afforded by hospital and dispensary outpatients. The simple expedient of keeping a ruled tabular note-book leads of its own accord, and without any serious mental effort or consumption of time, to most valuable results.

REVIEW III.

Dei Funghi Sospetti e Velenosi del Territorio Senese. Per FRANCESCO VALENTI-SERINI, M.D., &c. Pubblicato sotto gli auspicii e per cura della Reale Accademia di Medicina di Torino. Torino, Litografia Giordana e Salussolia. 1868. Pp. xx, 34. Tavole 56.

On the Suspected and Poisonous Fungi of the Territory of Siena. By Dr. F. VALENTI-SERINI, M.D. Oblong folio, with fifty six coloured plates.

IN the year 1864 Dr. F. Valenti-Serini, of Siena, presented to the Royal Academy of Medicine of Turin the manuscript of this very important work—important both in a botanical and in a medical sense. With the manuscript he also presented a magnificent atlas of fifty-six drawings, representing these deleterious fungi of the natural size, and in their natural colours; together with a series of tablets in relief, produced in terra cotta, correspoding with these drawings, likewise coloured, and of the natural aspect of the fungi. This rich collection, designed by its author to point out with the greatest distinctness the poisonous and the suspicious fungi of the Sienese territory, has been placed in a suitable Cabinet in the Museum of the Academy of Medicine, so as to be available to the public, and all those desirous of studying these dangerous productions. In this region of the peninsula fungi are abundant, and are frequently used as articles of diet, especially in Lent; consequently accidents arising from partaking of the poisonous ones occasionally occur. The ancient Romans designated fungi as *food for the gods.* The design of Dr. Valenti-Serini was most laudable, viz. to endeavour to familiarise the eyes of the population with these singular plants, and thus to prevent their use.[1] It may be added that nearly all, if not all, these dangerous fungi grow in the British islands.

The President of the Academy of Medicine placed Dr. Valenti-Serini's manuscript in the hands of one of the most active and accomplished members, Dr. Antonio Garbiglietti, one of the professors in the Medical Faculty of the University of Turin. This gentleman made a report upon it, which he laid before the

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[1] The author's labours have been constant in this division of hygiène. He had previously issued a collection of 112 tablets designed in relief, representing a Popular Hygienic Toxicological Cabinet, showing all the edible, suspected and poisonous species of fungi, which was accompanied with a descriptive treatise. These tablets reproduce the fungi in their different stages of development, and in all the changes of form to which they are subject.

Academy in 1864. In this report, which was of some length, Professor Garbiglietti recommended the publication of the manuscript, showing, at the same time, that he considered it to be of very great value and utility.[1] And, in order to extend its usefulness, he at the same time presented to the Academy a MS. catalogue of the principal species of fungi, including both edible and deleterious, growing around Turin and in the continental states of the former kingdom of Sardinia. This was a great extension of the field of Dr. Valenti-Serini's researches, so as to include Upper or Northern Italy. It was printed *in extenso*, in the 'Transactions' of this learned and energetic body, in 1867.[2] This catalogue gives the synonyms, a vastly important yet difficult matter in cryptogamic botany, the places of growth, the seasons, and remarks on the dietetical, or otherwise poisonous qualities of the fungi, with many other things, of 500 different species. It is a work which does great credit to its versatile and very able author.

The Academy of Medicine adopted the recommendation of Dr. A. Garbiglietti, and ordered the printing of Dr. Valenti-Serini's manuscript, with the reproduction of his drawings, in a series of excellent coloured lithographic plates, which are now before us, representing every species and many varieties of the poisonous or suspected fungi of natural size, with sections showing their structure.

The work consists of an introduction of twenty pages, which is followed by a full descriptive account of all the fungi depicted upon the plates, their distinctive characters, synonyms, &c., including a great many very instructive remarks.

In the introduction the author observes that the use of these plants as an aliment has been condemned by the wisest and most prudent physicians of all periods, and the vast numbers of fatal accidents reported by writers of every age, from Hippocrates to our own times, only confirm this condemnation. Forestus relates that the odour of some species has produced epilepsy, and also death. Dr. Valenti-Serini is not inclined to go so far, but tells us that the fetor of *Clathrus cancellatus*, which he has kept in the driest state in his herbarium for thirty years, clings to it still, and that the subject of his forty-fifth tablet, when he was engaged in drawing it, even with the window open, gave out such fetid exhalations as to produce vertigo and a degree of deliquium.[1]

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[1] 'Interno all' Opera Manoscritta del Dottore F. Valenti-Serini sopra i Funghi Sospetti e Velenosi del Territorio Senese, Relazione del Socio Dott. Collegiato Cav. Antonio Garbiglietti.' Torino, 1864.

[2] 'Catalogo della principali Specie di Funghi crescenti nei contorni di Torino ed in altre Provincie degli antichi Stati Sardi di Terraferma, disposta secondo il Sistema Micologico di Fries. Compilato per cura del Dott. Coll. Cav. Antonio Garbiglietti.' Torino, 1867, 4to., pp. 64.

It should be noted that some fungi, often of the same genus, have an external and internal structure almost alike, undergo many changes in their aspect and appearance in the course of their brief lives; so that the difference of these varieties and species are drawn from characters which are very inconstant and accidental, whence the same individual is known under different names. This confusion of names is a great source of evil:

> "It often happens that, in different countries, one and the same species of fungus is indicated by different popular names, and, what is worse, is that, under the same name, a fungus is considered to be edible in one country which in another is known to be poisonous. Lastly, the species or varieties of fungi placed among the edible are subject, from causes not yet understood, to those internal alterations which change their nature, and render them more or less noxious, and even poisonous.
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> "All this contributes to occasion fatal errors in the selection of fungi, and to render the use of even the edible species uncertain and dangerous, which species, in the estimation of the author, differ very slightly from the poisonous ones."

In support of this view, it should be stated that Dr. Garbiglietti goes so far as to say that nothing appears to him more logical than the admission that such species of fungi recognised as innocent, may for all that be susceptible, when placed in certain circumstances, of being converted into malefic ones. Such opinions of observers of great knowledge and experience are weighty aud momentous.

Dr. Valenti-Serini devotes short special sections to the origin of fungi, the organization of fungi, their division according to their properties, in which he makes three classes—*those of good quality*, *the suspected*, and *the poisonous;* to the chemical analysis of fungi, the question whether there exists such signs in fungi as to enable us to distinguish with facility the innocent from the noxious; warnings to excite one's suspicion in the selection of fungi, symptoms of poisoning by fungi, autopsy of those poisoned, what action do poisonous fungi display on the animal economy, and, lastly the curative treatment of those poisoned. Although the remarks of the author upon these various subjects are brief, and, it may be added, condense in a judicious manner what is known relating to these plants, we can here refer to two or three points only.

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[1] Dr. Garbiglietti, in his report, mentions that the author might have collected many other cases, proving the nocent influences of the fetid emanations of fungi; and refers particularly to the "Melethemata inauguralia" of Dr. Vincenzo Pico, of Turin, who relates that two robust maid-servants, on entering early in the morning a room in which a quantity of *Agaricus bulbosus* had been shut up all night, fell down in a fainting fit, and experienced vomiting, swimming of the head, and dimness of vision. Still Dr. Garbiglietti is not disposed to attribute the effects experienced in this important case to the offensive odours of the fungi solely. He says these plants differ from others in the absence of the green matter (*chlorophyl*), consequently they exert on the surrounding air a chemical action opposite to that effected by other plants, which absorb the carbonic acid exhaled by men and other animals and decompose it, retain the carbon and restore the oxygen to the atmosphere. Fungi, on the contrary, like flowers, fruits and the coloured parts of other vegetables, corrupt the air by exhaling large quantities of hydrogen, azote and carbonic acid gas. Hence, he is inclined to attribute the syncope of the maids rather to the corruption of the air, rendered no longer respirable, than solely to the fetor of the agarics.

Chemically, fungi may be regarded as allied to the animal kingdom, from the abundance of azote produced in their analysis, and from their decomposition, which gives products similar to that of animals. Many fungi belonging to different genera contain the active or poisonous principle, which Letteiller has named *amanitina.* But, as the same principle may be found in all these genera, as not rare instances of poisoning prove, and is not confined to the section of *amanita* only, the author would name this active principle *fungic poison*, or, adopting Letteiller's method, *Agaricina*, *Merulina*, *Hydneina*, *Verpina*, *Polyporina*, *Boletina*, *Phallina*, *Clathrina*, as in all these genera there are poisonous species. This last writer states that the deleterious principle of fungi is in its nature very fugacious, and therefore difficult to analyse. Dr. Valenti-Serini, seeing the little that is accurately known of the composition of fungi urges chemists to new researches, and considers that these will ere long lead to the discovery of the nature of their poison, which he surmises, and gives his reasons for thinking, will probably prove to be a greater or less quantity of prussic acid. In the present advanced state of vegetable chemistry there is every reason to expect some decided progress to be made in this curious subject of research, when competent analysists shall direct their attention to the inquiry.

In more northern counties, in which fungi are collected without distinction, it is usual to macerate them for some time in water, renewing it frequently, or, better still, to place them in salted water, or to immerse them in boiling water, as it is known by experience that boiling dissipates many of the principles of these plants, and alters their properties. By these means disastrous accidents become very rare.

When speaking of the signs by which to distinguish wholesome from deleterious fungi, the author avers that they are much more proteiform than other plants, and in the course of their lives are subject to so many and such varied changes as are scarcely to be credited. "Sunt mala mixta bonis, sunt bona mixta malis." He refers under this section to his popular hygienic toxicological collection, before named. He gives a number of rules, embracing the places of growth, the sensible properties, the appearances, the age, and condition, &c. of fungi as warnings against suspected species, adding, lastly, that it is advisable to make experiments upon dogs, cats, and other animals, to test their properties. Still, he holds it prudent to abstain altogether from these plants, which, although nutrient, are always little or not at all salubrious, often pernicious, and not seldom fatal.

In the section upon the symptoms of poisoning occasioned by fungi, the author states that—

> "The poison of fungi, of whatever kind, is most powerful, and, according to Zeviani, contains in itself the malignity of all poisons. Still, in its action upon the economy it varies according to the species taken, the quantity ingested, the sensibility of the individual, his temperament and constitution, and to the time at which it is taken.
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> "Some species, in themselves not poisonous, but of hard texture, when taken in moderation, occasion only disorders of stomach, diarrhœa, with pains in the belly. Such are the *Cantharelli*, called in Tuscany *Giallarelle*, of which Scopoli, in his 'Flora Carniola,' says, 'Chantharellus edulis; sed coctura, et condimentis rite paretur; alias tormina, et diarrhœas excitat.'
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> "Other species produce stupor and transient delirium; whilst some, and not a few, act as decided poisons, developing a violent irritation and an inflammation of the gastro-intestinal canal, so acute as to pass into a speedy and more or less extensive gangrene. The symptoms by which the poison manifests itself are the following: —nausea, efforts at vomiting without being able to vomit, swoonings, frequent anxiety, a sense of suffocation and of oppression, a sense of burning with thirst, constriction of the throat, continued gnawing, raging pain at the stomach; sometimes frequent and violent vomiting, simultaneously with abundant blackish or sanguinolent ventral dejections, accompanied with colics, tenesmus, and painful tension of the abdomen. In other cases there is retraction of the umbilicus and alvine constipation, united to the symptoms above described.
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> "Aggravating the malady, there are present a weight in the head, vertigo, stupor, delirium, drowsiness, lethargy, painful cramps, convulsions in the limbs and face, marble coldness of the extremities, irregularity and smallness of pulse, the Hippocratic aspect, and, lastly, death puts his seal to so many sufferings.
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> "According to some authors, the mental faculties remain unaffected to the last moments; according to others, they are obscured.
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> "Phenomena so fearful manifest themselves in some cases soon after the injestion of the fungi, but more frequently after ten, twelve, and even twenty-four hours, which is much worse, since after such a time the poison is diffused in the general organism, and renders every curative attempt vain."

Speaking of the morbid appearances, the author states that—

"The integuments of those who die of poisoning occasioned by fungi are observed to be covered with violet spots more or less extensively; the belly is most voluminous; the conjunctiva injected; the pupils contracted; the stomach and intestines inflamed and covered with gangrenous spots, and sometimes sphacelated in portions of the stomach, with great contraction of the stomach and intestines, thickening of the membranes, and obliteration of the canal. The œsophagus phlogosed, and also gangrened and in some cases the ilium invaginated from above into the lower portion. The lungs inflamed and engorged with black blood; and the same engorgement in almost all the veins of the abdominal viscera, as the liver, the spleen, and the mesentery. There are seen marks of inflammation and gangrenous spots on the membranes of the brain, in its ventricles, on the pleura, lungs, diaphragm, mesentery, bladder, uterus, and also upon the fœtus of a pregnant woman, with the blood in a state of fluidity in this case, almost coagulated in others. In one individual, according to Orfila, the intestines were found to be filled with fæcal matters. In no case were there vestiges of the fungi found, because these had been digested or evacuated. Extreme inflexibility of the limbs was not found to be constant."

The author agrees with Orfila that the greater part of fungi operate in the manner of *irritant* poisons, or of *narcoti-acrid* poisons.

The curative treatment consists in the speedy expulsion of the fungi by means of emetics and purgatives, by decided doses of ipecacuanha and tartar emetic, to which may be added sulphate of soda, of potash, or of magnesia, given in small doses at brief intervals; if necessary, full doses of sulphate of zinc, followed by copious draughts of warm water. If the purgatives are slow in producing their effects, recourse may be had to infusion of senna, castor-oil, and other medicines, together with glysters. Where the fungi have been taken some hours, and have begun to manifest their effects, the author recommends a combination of the emetics with cathartics, in full doses.

As these deleterious plants are often eaten in country places, where medical men are not at hand, and where the appropriate remedies are out of reach, Dr. Valenti-Serini advises immediate recourse to popular means. First, the fauces should be titillated with the finger, or an oiled feather, to provoke vomiting, or the patient should, especially if not accustomed to its use, be induced to smoke tobacco. Some have proposed a decoction of tobacco, made with a pint of water, to be used in lavements—a remedy of very questionable character. To excite vomiting, a decoction of the root of the sweet violet (*Viola odorata*, Linn.) made by boiling a third of an ounce in half a pint of water down to three ounces, or the powder of the root, dividing an ounce into twelve parts, and giving them at short intervals until full vomiting is induced. As this root excites vomiting only, there may be taken, at the same time, an ounce and a half of common salt dissolved in water, which solution may also be used for injections.

After the material cause of poison is removed, where symptoms of acute irritation remain, mucilaginous remedies united to sedatives should be employed, as decoction of mallows, of linseed, or of rice, gum water, barley water, milk, or oily potions, opium with its preparations, and also ethers.

If afterwards there should be fever, with painful distension of the belly, dry red tongue, great thirst, hot burning skin, smarting in the throat, and other symptoms, which show the existence of a state of inflammation; the use of strong purgatives is to be intermitted as unsuitable, but instead, fomentations should be used and linseed poultices applied to the belly constantly, a general warm-bath, the repeated application of leeches to the body or the hæmorrhoïdal vessels, and a general bleeding is necessary, which may be repeated according to the forces of the individual and to the more or less intense state of the malady. If the evacuation of the material of the poison is not effected by either vomiting or purging, it is of importance that the use of acids, common salt, and ether should be withheld, as these commonly aggravate the ailments. Orfila showed that all these were solvents of the active part of the fungi, the solutions becoming energetic poisons.

At the present time, when one of our principal and most useful societies is engaged in collecting and diffusing information to show that our fungi, with few exceptions, and these easily discriminated, may safely be eaten, it is particularly important that attention should be drawn to Dr. Valenti-Serini's labours and to this fine volume. It is impossible to quote his authority, who has devoted so many years of his life to the study, in support of either of these positions of our Society of Arts.

It is the business of physicians to be the benefactors of their times, and to diffuse blessings around them. This work of Dr. Valenti-Serini is a prominent instance of the kind, in which an able man has devoted the energies of his mind most perseveringly to benefit his countrymen, by spreading such an accurate knowledge of suspected and poisonous fungi among those who consume such articles of diet as will effectually prevent their use, and the noxious effects which ensue. The Royal Academy of Medicine of Turin is entitled to the warmest commendation for promoting such a contribution to the cause of science and humanity, which well deserves to be known much beyond the confines of Italy.

REVIEW IV.

Comparative Anatomy and Physiology of the Retina.

IN an article on the structure and functions of the human retina, contained in a recent number of this Review, we gave a summary of some earlier researches, together with such brief outline of the progress made by modern anatomists as the space at our disposal would allow. The great amount and diversity of details (scarcely, indeed, to be compassed within the ordinary limits of a review) precluded all discussion of their physiological significance; and, consequently, the consideration of this portion of our subject was reserved for a future occasion. But the interpretation of the numerous anatomical facts placed on record trenches so closely upon explanations given (or attempted) of the visual apparatus in other animals as to cover a far wider field of inquiry than that occupied by the physiology of vision in man. Before, therefore, the subject can be treated in all its bearings, some preparatory account of the retinal apparatus and accessory eye-structures found in various classes of animals appears indispensable. For it is obvious that vision is effected in many animals under different conditions, and, apparently, by different means, so that, apart from the question of varying amount and perfection of vision, there has been much debate respecting the identity of physiological basis, as well as unity of anatomical structure. The wide-reaching import of such an inquiry necessarily involves an equally wide and careful scrutiny of the variously differentiated structure of the retinal organs in all animals possessed of a faculty of vision.

But in thus extending our anatomical basis, another necessity arises—that, namely, of determining the homologies of the several constituent parts of the eye in different classes of animals. And to determine these it is needful to compare the development of the separate tissues and retinal elements, so as to establish their histologic equivalence, and to assign to each its special place and function conformably with general experience. These homological relations can, of course, only be established satisfactorily when the development history of the eye structures in each of the individuals to be compared has been previously studied. The comparative anatomy of the organs of vision, therefore, includes the study of embryonic, as well as adult, structures.

But it is not necessary to our present purpose to enter into any embarrassing details of comparative anatomy, or to obstruct the general physiological argument by introducing minor questions of homology, which are still *sub judice*, and, therefore, inapplicable to the solution of the main problems of retinal function. Our statements will be limited strictly to such facts as are well attested, and bear directly on our argument.

A satisfactory theory of vision should, without any violent reversal of fundamental physiologic principles, be equally applicable to every known variety of visual apparatus. But as, in all cases of complex mechanism, those parts only can be considered essential which are found to be constant and invariable, the determination of such essential parts can be effected only by an extended series of comparative observations. We thus ascertain, as a simple matter of fact, what are the limits of differentiation, and whether the difference exists in the separate elements or in the arrangement of tissues in the composite whole. But again, we cannot accomplish this, without first tracing the development of the several parts in each individual case. Our anatomical analysis is as unsafe, where the homological relations are not established, as our physiological inferences would be unsound without the guidance of general physiological principles.

In a morphological point of view, comparative observation, to be serviceable, requires a standard of comparison; and the selection of the vertebrate type for our standard, whether accidental or arbitrary, has obvious advantages. In a purely physiological sense it might, indeed, seem more methodical to begin with the study of the simplest apparatus, as presenting a less complicated problem, both of structure and function. The descriptive anatomy of so complex a structure as that of the human retina presents many difficulties, the clearing of which is greatly facilitated by examination of the various kind of apparatus found in the descending scale of animals. Nevertheless the vertebrate type of eye happens to be the one most studied and best understood, and consequently most convenient to retain as a standard of comparison. Moreover, the fact, that we derive our idea of a visual function entirely from our own sensation and experience of what takes place in our own organs, makes these the most suitable objects of our preliminary study, and therefore most productive of practical results. To keep this our standard type in full and undisturbed view, it is necessary to consider it at first (as we have done in our previous article) apart from the various modifications of structure observed in other animals; for our particular conception of a function gives a special direction to anatomical research, and concentrates attention on many minute details, which would probably be overlooked but for the importance with which *à priori* reasoning invests them. In the case of the sensory organs, the postulates derived from previous knowledge of material and sensorial phenomena are peculiarly exacting, and instead of accommodating themselves to the errors of an imperfect anatomical analysis, requently contradict them, and provoke fresh examinations, leading to better knowledge and a more accurate estimate of the facts. This remark applies with especial force to the history of retinal anatomy. Experience has conclusively shown that the full significance of our anatomical researches can be rightly comprehended only when we know the real nature and exact extent of the function which the organ under examination subserves. Thus the great laws of physiological optics have not only guided the anatomist in his dissections, but apply also with a *corrective* force to the hypothetical interpretations advanced by those who undertake to explain their discoveries. On the other hand there is a peculiar danger (as we shall have occasion to show in our present article) in the self-assertion of scientific dogma when it previses and preoccupies the field of research which properly belongs to the anatomist. Thus our knowledge of the physical conditions pertaining to optic phenomena, and of the physiological actions of the parts which concur to their production and regulation (as exemplified in the theory of accommodation and the anatomy of the ciliary muscles with the connecting links of sensation and innervation) is far in advance of any speculations that we might make respecting retinal sensation, and the mechanism by which it is brought about. The physiology of retinal action is too imperfect to allow of any exact prevision of a *modus operandi.* It is far more likely that diligent observation and careful verification of anatomical facts (by extended comparison of the eye structures in various animals) will help us to a conception of retinal function, than that any scheme which we might derive from *à priori* notions of this function will supply the deficiency of anatomical research; and even the errors caused by hasty generalisation from incomplete series of anatomical facts, are less obstructive and misguiding than the principal mistake of a false axiom in physics or physiology.

Nevertheless, in so difficult an investigation, fact and hypothesis must march hand in hand for mutual support, provided always that hypothesis be employed only for present need, as builders use a scaffold for the raising of materials and the convenience of the workmen.

For so long a preface some apology may appear due to our readers. It will, perhaps, suffice to remark that our object is not so much to review any particular work, as to indicate the general state and progress of retinal physiology.

Referring to our former article for the descriptive anatomy of the human retina, we now proceed to discuss the doctrine taught in our schools, and laid down in our text-books, respecting the distinctive characteristics of the visual organs and function in the two great provinces of vertebrate and invertebrate animals. It is generally held that there are two distinct types of construction, and that the act of vision is effected in these types on different, and, indeed, opposed principles of optics. Johan Müller, who formularised and imparted to this theory of a double type its seemingly precise and scientific character, was the first to show that the older anatomists erred in supposing that either of these types was exclusively found in the respective provinces of vertebrata and invertebrata. His dissections demonstrated that the simple or accessory eyes of the invertebrata were constructed on the so-called vertebrate "type," and this demonstration at once swept away the presumed distinction of character in the vision of the two classes of animals.

It may be as well here to indicate, as briefly as possible, the differences between the two "types" of construction. The vertebrate type is represented by a hollow globe, formed of one or more membranous envelopes, its interior being filled with transparent refracting media, in front of which light is admitted through a transparent portion of the external membrane of the globe. The central mass of refracting substance (the vitreous body) is overlaid by an expansion of nerve-fibres and associated elements (composing the retina), just as an ivory ball is enclosed in its cup. The retinal expansion, in its turn, is overlaid by a membrane, whose surface of contact is darkened by a lining of pigmented cells. The action of the refracting media, by which a picture of external objects is formed on the internal spherical concave of the pigmented membrane, is commonly illustrated by likening the whole dioptric apparatus to a camera obscura, the images formed by which, fall on a ground-glass plate, or, as in a photograph camera, on a chemically sensitive surface interposed at the focal plane.

The "invertebrate type," on the contrary, is *not* that of a globe filled with refractive media; and the optic nerve, instead of being spread over a "vitreous humour," is continued forwards to the front of the eye as a conically enlarging mass of fibres, granules, and nuclei, amidst which connective tissue, pigment layers or spots, tracheal tubes and blood-vessels are arranged in definite order, filling up the interior of the eye. Since, therefore, it is impossible that images of external objects can be formed by the passage of collective pencils of light through the interior of an eye thus constructed, it was concluded that the sensation of light was excited by direct contact of rays penetrating the cornea and falling on the nerve-fibres without the intervention of any optical image. The anatomical disposition of the parts of the compound eye (as in insects, &c.) was in short interpreted, as implying a different application of optical principles, and even a different law of sensation.

In respect to the simple eye of insects, &c., J. Müller had, as already stated, come to the conclusion that it closely resembled the eye of the fish, having a plain corneal surface, a single globular lens, and an optic nerve expansion, on which images of external objects could be formed by collective pencils of light passing through cornea and lens, and thrown focally on the retina. Here, therefore, the same mechanism and optical principles apply, as in the eyes of vertebrate animals.

In the compound eye, on the contrary, another mode of vision was propounded. Each separate facet of the compound cornea was supposed to admit a central pencil of isolated rays, which, penetrating in direct lines, reached the ends of the optic fibres opposed to them, producing a multitude of separate impressions, and as we see a pattern worked in mosaic composed of numerous small inlaid pieces, so the image of an external object was supposed to be made up of the separate impressions caused by rays of light proceeding from the illumined points of the object seen. This mode of vision J. Müller called "mosaic vision."

The theory propounded by Müller, that a dual type of construction, and a corresponding duality of physical and physiological function existed in one and the same animal, is of itself open to grave suspicion, as no other instance can be adduced of a highly specialised function being performed in two opposite ways with the same object and result. If the existence of two distinct types of organisation in one and the same animal, and subserving the same function, were indisputably proved, and if this supposed quality admitted of no other explanation (such as successive stages or constructive modifications of one type), this single fact would outweigh all the arguments in favour of a fundamental unity of type in the animal organisation, which have lately been set forth with such precision and supported by such circumstantial evidence.

Darwin himself, in his theory of a genealogical continuity of animal structures, distinctly recognises the serious difficulties, which the alleged existence of a double type of the organ of vision in articulata, would throw in his way. For no differentiation of organs consequent on "natural selection" could initiate so fundamental a difference of physiologic principle as is implied in Müller's theory of a double type of visional function. As a matter of fact, indeed, the variations of external conditions under which vision is exercised, are not so great as to require such change of principle. On the other hand, the assumed divergence of type would cut into irreconcileable fragments the theory of "unity of type" and continuous descent and kinship of animals.

It is therefore especially worthy of notice that Johan Müller, who discerned and demonstrated the true correlation of the structure of the simple eye of articulata with that of the vertebrate eye, did in large measure reform the erroneous doctrine founded on inaccurate anatomical premises, and in equal degree prepared the way for acceptance of the new views that have been slowly indeed, but always more securely, establishing themselves. And it is further to be noticed that Müller's hypothesis of "mosaic vision" was invented to meet a difficulty in physiological optics, occasioned by the apparent anomaly of a "compound" eye, rather than founded on any new discovery of retinal structure and function. Nevertheless, this theory remained undisputed for more than twenty years, although Leuwenhoek had experimentally shown that every facet of the compound cornea supplied its own separate image of external objects, and that, not one, but a multiplication of images resulted. If we look through a transparent insect cornea, we see as many images of an object in front of it, as there are separate facets; looking, for instance, at a man, we see, as an old writer remarks, a whole army of dwarfs. How is it possible, under such circumstances, to imagine "mosaic vision?" If, again, an outlined image were not formed by each corneal facet, how can we conceive it possible, that the isolated nerves behind the facets should be capable of transmitting any definite impression of form and shape, or of mapping together the surface details of an object without confusion of boundaries?

The hypothesis of mosaic vision is disposed of by the well attested fact, that a multiplicity of optical images is really produced by the facetted cornea. But even if this were not so, the hypothesis fails to render intelligible, how isolated impressions, excited by rays of light passing from a number of illuminated points and reaching certain nerves, should combine to a representation of the whole object, since the action of such an apparatus might rather be expected to result in the appearance of a number of single details, that is, of separate objects, than in the production of an optical image having continuity of parts. Moreover, the problem to be solved, namely, how single vision results from multiple images belongs equally to all organs of vision (the vertebrate type and man himself included); and the true conception of a faculty of vision is not to be gained by ascertaining certain differences in the construction of parts concerned only in the dioptric mechanism, but by determining the identity or otherwise of the retinal function in all animals possessing vision. Our present knowledge of retinal structure tends towards the hypothesis of fundamental unity of type, where the compound eye of articulata suggests neither an abnormal differentiation (monstrosity), nor any degradation resulting from external agency.

Göttsche, who repeated Leeuwenhoek's experiments was the first to express doubts of the accuracy of Müller's theory ('Archiv,' 1852). Leydig, after careful dissection of various arthropode eyes ('Archiv,' 1855), came to the conclusion that many morphological facts, besides the demonstration of the lens shape and action of the corneal facets, were directly opposed to the hypothesis of separate types, and that instead of typical differences, remarkable analogies and structural resemblances occur. Leydig, however, expressed himself then with the reticence inspired by his respect for the great authority of the celebrated physiologist who had contributed so much to our knowledge of the sensory organs and functions; and agreeing cordially with the felicitous eulogy of Du Bois-Raymond, "that to have suggested a scheme by which nature, if she had been pleased to work according to its indication, might have constructed an eye capable of seeing clearly, must ever be considered a subtle stroke of genius, revealing a profound insight into the conditions pertaining to the exercise of a faculty of vision."

The continued attention which Leydig has paid to this subject has enabled him to confirm his assertion of "unity of type" in the eye structures, by a number of original observations, and the facts which he has brought forward, entitle him to rank amongst the first authorities on retinal anatomy.

The multiplication of images of objects by a facetted cornea is so intimately connected with a right understanding of the retinal mechanism of insect vision, that it is necessary here to enter a little further into the history of certain anatomical discoveries bearing upon it. J. Müller maintained that images were formed not *by* but *behind* the cornea, and that such images were formed by a number of small spherical, highly refractive bodies, which he considered to be crystalline *lenses* interposed between the cornea and nerve-fibres. Most of the earlier anatomists had noticed them (Swammerdam, Leeuwenhoek, Carolini, &c.); and Müller concluded, from his first studies of the insect eye ('Zur vergl. phys. d. Gesichtsinnes,' 1826), that these "lenses" were present in the eyes of all insects and crustacea. Treviranus, however ('Beitrag,' &c. 1828) objected that they were not universally present; and on further examination, J. Müller assured himself that exceptions existed ('Archiv,' 1829) in Dipterous insects. He found and described them in various crustacea ('Zeitschr. f. Anat. u. Phys.'), and supplied an omission in Straus-Durckheim's essay, 'On the

Anatomy of the Chafer,' in which the author had not mentioned these crystal bodies. At a later date, Will ('Beitrag z. Anat. d. Zusammenges. Augen,' 1840) succeeded in demonstrating the crystal body in all species, including Diptera.

Meanwhile, a question arose concerning the relation of these bodies to the optic nerve-fibres. Cuvier had expressed an opinion that they were actual continuations of the nerve-fibres. J. Müller, who had chiefly examined those eyes in which the crystal bodies are so large, and appear, with the usual modes of preparation, so distinct and separate, and withal so refractive, that the notion of their being composed of nerve-substance might well seem extravagant, affirmed them to be lenses, and sought to prove that only the rays of light falling axially through them could penetrate to the nerve-fibres behind. Rudolph Wagner ('Archiv f. Naturgesch.,' 1835) concluded from his researches, that the nerve-fibres not only reached the crystal body, but also spread themselves over their outer surface, and formed a sort of capsular envelope (or *retina*) round them (Sphynx Atropos). This physiologist interpreted the facetted eye to be an agglomeration of numerous single eyes, and considered that all the rays which fell on the crystal lens, were focussed on the interior of the retinal capsule described above, and therefore that vision was effected as in the vertebrate eye.

Leydig takes an entirely different view of the morphological relation of the crystal bodies to the optic nerve-fibres. In his essay "On the Eyes of Arthropoda" ('Archiv,' 1855) he describes the varied form of the nerve-ends, and includes the crystal substance as a constituent part of the nerve-termination. The shape of this crystal body had been previously described as spherical or pear-shaped, with its stalk directed to the fundus of the eye. Göttsche (1852) had, however, recognised in the crystal body of the eye of the river crab, a double pyramid shape, ribbed longitudinally on the outer edges, or "prism-shaped columns," whose substance exhibited transverse striations. Leydig represents them more accurately as prisms composed of four segments fitting lengthwise together, with notchings and bulgings of the ribbed sides, and their ends likewise as being knobbed or clubshaped, with four lobes corresponding to the segments. Sometimes the ends are closely pressed against the posterior surface of the corneal facets, and show a depression in the centre, in which the convex projection of the lens-shaped corneal facet rests. This structure is, according to Leydig, the actual termination of the nerve-fibre, and is formed by four cylindrical cells corresponding to the four segments above described, the contents of which gradually differentiate and become more refractive. Other anatomists, however, consider the crystal body to be formed by an exudation from cuticular cells.

The morphology of the nerve-fibre itself is almost as variously interpreted as that of the crystal body. Swammerdam recognised the fibres, but could not satisfy himself as to their arrangement and termination. He remarks that they might be compared with six-sided prisms, increasing in thickness towards their outer ends; and obviously includes in his description of nerve-fibre the crystalline substance in which, according to Leydig, it terminates. Other anatomists describe them simply as clear threads running forwards in a partly or wholly pigmented sheath, to meet the inner end (or stalk) of the club or pear-shaped crystal bodies. J. Müller recognised the fibre as an ordinary nerve-tube, with mark substance and axial fibre. Will, in 1840, noticed the nerve-fibres as clear lines running axially through the club-shaped mass of crystalline substance. But, in 1843 ('Archiv') he treats his former observation as an error, and occupies himself with the description of a motory apparatus of muscle-cylinders arranged round the nerve. Leydig showed that these muscle-cylinders were nothing more than the longitudinal ridges already mentioned, as marking the junction line of the segments composing the club-shaped crystal bodies. Even in the eyes of Hymenoptera and Diptera, the nerve-fibres have, says Leydig, a markedly quadrangular shape towards their outer end (in section showing the form of a cross), but the longitudinal ridges are not so pronounced, as in the eyes of certain other insects and crustacea. But other forms of this club-ended nerve are described by Leydig. Sometimes it is a pyramid with ridges, having a curved outline; sometimes it is a simple four-lobed cylinder; sometimes a minute but still four-knobbed particle; or the club-shaped swelling may be continued into a quadrangular thread, and this latter into the substance of a crystal body in front of it. Again, the nerve is not always one thread, but often subdivided into several fibres (as a piece of rope untwisted separates into its component strands).

The optic nerve-fibre is considered by Leydig to be directly continuous in substance with the club-shaped expansion and crystal body itself; and the whole structure, therefore, homologous with the bacillary structure of the vertebrate retina. Having first assured himself by experimental tests, that in respect to physical characters and refractive property, these crystal-looking nerve-ends agreed with the large retinal bacilli of the eyes of amphibia, and being particularly struck with the existence of the same rose-tint colour in both, he found, on further examination, that in many insects the nerve and the crystalline body formed one continuous whole without anatomical break, or any change in physical and chemical properties. More often, however, the club-ended nerve is so soft as to appear like a transparent jelly, which does not exhibit its characteristic four-lobed shape until hardened by preparation. To this circumstance Leydig attributes the failure of Treviranus, Müller, and others, in not having at first recognised the constant presence of the crystal body. The jelly-like condition obtains in hymenoptera, diptera, and some orthoptera. Another condition is that in which the substance of the crystal body differentiates into parts of unequal consistence and refractive power. If the crystal body is globular, its centre looks like a nucleus, and its surface appears as a soft investing layer, continued back into a stalk, which gradually thins into the nerve thread. The whole appearance then resembles that which is interpreted by Wagner as a lens enclosed in a nerve or retinal envelope. In other instances (certain fresh-water crabs) the nerve-end differentiates into a crystal body, whose interior has a jelly-like consistence. Sometimes the crystal body is so minute as to escape notice very easily (as in Vanessa Atalanta), unless specially searched for; and appears, when found, as a very small nucleus enclosed in a sheath, which stretches itself as far as the cornea, under the guise of a simple cylindrical rod. In Sphinx convolvulus the crystal body is large, and the central substance slightly more consistent than the superficies; but the internal core and the external layer are composed of the same substance. The nerve-fibre, with its crystal end-swelling, is enclosed in a fine membranous sheath, composed of areolar tissue, and which Leydig compares with neurilem or sarcolem. Nuclei have been observed on the inner side of these investing sheaths, which the same observer considers to be remnants of the connective-tissue matrix, from which the structureless envelope was formed.

Claparede ('Zeitsch. f. Wiss. Zool.,' 1860) sharply criticises Leydig's opinion respecting unity of type in the organisation of the simple and compound eye, and especially objects to Leydig's morphological interpretation of the crystal bodies. Claparede holds with Müller, that the crystal bodies are "lenses." To this view Leydig opposes the general fact that corneal images are produced independently of any refractive action of the crystal bodies behind the cornea. Müller urged the exceptional occurrence of insect corneæ possessing facets so curved as to act like lenses, and Claparede repeats the argument. Leydig, on the contrary, maintains that this argument has lost all force, since he has demonstrated that the compound cornea is, in the large majority of instances, composed of facets having distinctly appreciable curvatures. He finds the corneal facets sometimes curved on their anterior, sometimes on their posterior, surface, and often on both; and argues that the refractive action of the crystal bodies, so far as the formation of images is concerned, is superfluous. The function attributed by Leydig to the crystal bodies corresponds with his interpretation of their homology with the bacillary stratum in which the optic nerve-fibres terminate in the vertebrate eye; that is to say, they are the light-perceiving elements; but he suggests that they combine the properties of refraction and perception. In his treatise on the eyes of articulata (1864), he makes the following remark:

> "Strictly speaking, the same uncertainty prevails respecting the retinal cones and rods of vertebrate eyes; their principal function, as every one admits at the present day, depends on their light-perceiving property; but who will absolutely deny that their light-refracting property co-operates in this function? Who can affirm positively where refractive action ceases and the perception of light begins?"

Claparede seems, after re-consideration, to admit that Leydig's comparison of the optic fibres of the insect eye and their termination in the crystal bodies with the retinal fibres and bacillary stratum of the vertebrate eye is to a certain extent sustained by his own observations on the eye, of Hyperia. Here, and in a few other cases, he finds the nerve-character of the crystal body to be highly probable. On the other hand, he adduces Leydig's own description of the chitinized crystal body in Elater, Lampyris, and Cantharis, as evidence against his views. To this Leydig replies, that the peculiar appearance in such cases may be fairly explained by supposing that the original nerve-substance had undergone conversion by a process of chitinization, such as he had formerly shown to have occurred in connective tissue, and even muscle-substance, and that there is no improbability in the supposition that nerve-substance may undergo a like conversion.

Gegenbauer ('Archiv,' 1840) also observed in Hyperia, crystal bodies, which prolonged themselves by a sort of stalk into a smaller thread or fibre, which he traced back into the optic ganglion. So far, therefore, Leydig's view is corroborated by independent observation.

The general position maintained by Leydig against Müller's representation of a double type is this:—In the simple and compound eye alike the optic nerve forms a central ganglion, from the periphery of which fibres run forwards in a radial direction and terminate in the stratum of club-shaped elements, which appear to be homologues of the bacilli of the vertebrate eye. The act of vision is performed in the same manner in both kinds of eye by means of these terminal club-shaped nerve-organs.

In comparing the general arrangement of the component parts of the simple and compound eye, Leydig comes to the conclusion that the first is but a modification of the latter. To follow up his demonstration, it will be here necessary to consider how far the analogies of the component structures in each are supported by anatomical evidence.

In respect to a sclerotica, although its subordinate and accessory character renders any determination of its homology comparatively unimportant, Leydig shows that in the simple eye the neurilem of the optic nerve is continued over the "optic ganglion" as an external tunic. In the compound eye this membrane is more developed, invests the optic ganglion with a strong and stiff capsule, and finally loses itself in the cornea.

The disposition of the cornea is more important. In both kinds of eye it appears as a translucent segment of the chitinous skin; in both, also, thickening of the corneal substance concurs to the formation of lenses; but while in the compound cornea the surface is partitioned into a number of small facets generally corresponding with the number of nerve-ends, in the simple eye the cornea, instead of being thus facetted, becomes, by a general thickening of its substance, a single lens.

There are, however, compound eyes (*e.g.* river crayfish) without facets; and there are simple eyes in which the cornea is concavo-convex, in which latter case (caterpillars, woodlouse, &c.) a separate lens is found behind the cornea. The size and outline of the facets is so various, and colouring pigment so irregularly deposited in its substance, that no importance can be attached to these points; but in many arthropode and insect eyes the surface is divided into four planes by cross (diagonal) lines, corresponding with the four-lobed division of the club-ended fibres of the optic nerve.

The optic "ganglion" of the compound eye is a complex structure, and distinguished by important peculiarities. The peripheral mass of interwoven fasciculi, from which spring the fibres that terminate in the before described club-shaped ends and crystal bodies, contains a greater number of fibres than are found in the optic trunks. The latter, entering at the bottom of the eye, consist of fibres which diverge as they run forwards, without anastomosis until they reach the central plane; they then break into fasciculi, which interweave in a continuous plexiform manner, the interspaces being filled with a granular mass, in which nuclei and cell-elements abound. A section of the conically enlarging "ganglion" exhibits a fan-like form, and a concentric lamination is observable, which indicates the addition of fresh elements, just as in the section of the vertebrate retina. The multiplication of nerve-fibrils is so obvious, that a whole group of peripheral fibres must be represented by a single optic nerve-fibre. The same numerical preponderance of rods and cones with their fibres over the fibres of the optic nerve, is observed in the vertebrate retina, and the "optic *ganglion*" of the invertebrate eye corresponds in this respect with the nerve and granule layers of the vertebrate *retinal* ganglionic structures. This fact suggests a possible explanation of the mechanism by which the reduction of multiple optical images into a single mental picture might be effected. Thus in the compound insect eye a sort of stereoscopic fusion of many images of the same object may be imagined to take place, as also the combination of different objects in one continuous picture, although placed at such angles of divergence as to occupy opposite sides of the hemispherical cornea. While in the vertebrate retina, the reflection of multiple images from the innər concave of the pigmented choroid might be imagined to undergo a like reduction and fusion in course of their transmission from the numerous cones and rods through the granule layers back to the relatively few optic nerve-fibres. And whenever simple insect eyes are aggregated together, the reduction of many impressions into one may be explained in the same way.

The optic ganglion of the insect eye is further distinguished by the occurrence of pigment deposits. Outside the sclerotic capsule the optic nerve-fibres are often sheathed in pigment deposited in patches, and sometimes a dark zone of pigment, collected in the connective tissue-cells of the neurilem, lines the posterior surface of the sclerotica at the bottom of the eye; but, besides this, pigment spots are found in the optic ganglion, as well as at the roots of the optic nerve, and mixed with them. Leydig has also seen particles of some strongly refracting substance closely adhering to the pigment, and which he interprets to be remains of the larval eye of the insect. This interpretation he supports by a series of observations on the eyes of perfect insects (beetles), in which he found, close to the compound eye, minute structures composed of nerve-stalk, pigment, and a crystalline substance, which he considers to be arrested developments, and which stikingly resemble rudimentary single eyes. These observations, if correct, greatly strengthen the proof of close organic relationship between the simple and compound eye.

The "optic ganglion" of the simple eye is necessarily less complicate than that of the compound eye; but, small as it is, its structure closely resembles that of the compound eye, consisting of a central molecular mass with minute peripheral nuclei, and an interwoven meshwork of optic nerve-fibres. In both kinds of eye, therefore, this ganglion represents the essential structure of the retina in vertebrate eyes, the elements being arranged in definite order, and exhibiting in section the same concentric lamination. It is, however, difficult to trace the nerves into their terminal organs. On examining with a low power the anterior surface of the pigment which encloses the optic fibres at the back of the eye, a layer of greyish transparent matter may be seen. Under a high power this resolves itself into a peculiar structure, consisting of closely-packed, pear-shaped bodies, of a jelly-like consistence, whose stalk appears to sink into the pigment mass behind. The oval or pear-shaped bodies fill up the space between the pigment at the back and the corneal lens in front. This structure Müller pronounced to be a "vitreous body;" Leydig, however, considers it to be a mass of radial optic fibres with their characteristic crystalline ends. Müller's interpretation is certainly not justified by the morphological appearances; and though the position of this structure corresponds with that of the vitreous body in the vertebrate eye, histological facts speak strongly for Leydig's view. In a physiological point of view the absence of a vitreous body in the insect eye is more readily accounted for than would be, the absence of a properly developed percipient element forming part of the nerve apparatus.

According to Leydig the only element which altogether fails in the arthropode eye, and whose absence especially distinguishes the invertebrate eye is the 'vitreous body.' If this were interposed as a central mass between the cornea and optic ganglion, the apparent divergence of type would be less striking, but the essential character of the act of vision would not thereby be changed. The absence of the vitreous body is compensated by the relatively large development of the crystal bodies (equivalent with the vertebrate bacillary stratum), and also by the close approach of the optic ganglion and its club-shaped terminal fibres to the corneal lenses in front; by which arrangement the corneal images fall directly on the percipient ends of the nerves.

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But additional proof of the close analogy between the structure of the invertebrate and vertebrate eye may be found in the consideration of certain important accessory organs. A choroid pigment membrane and an iris are indispensable parts of the vertebrate eye. The choroid coat of the typical vertebrate eye is so disposed as to invest the exterior spherical surface of the retina. In the invertebrate eye the choroid pigment is so distributed as to appear at first sight irreconcileable with the arrangement observed in the vertebrate eye: being either massed in the bottom of the eye around the optic fibres and entirely sheathing them, or lying in zones running through the solid interior mass of the optic ganglion. But if an insect eye (butterfly or beetle for instance), be cut in two halves and the cut edges carefully examined, there may be seen under the sclerotic capsule, a true choroid pigment layer, of which the zones of pigment distributed through the interior are offsets. To the whole of this pigment structure Leydig gives the name choroid which is in all invertebrate eyes intimately interwoven with the nerve-structures. Moreover, in addition to this, a separate pigment zone is found close to the plane of the crystal bodies (a stratum bacillorum of Leydig), which may be considered the analogue of the iris of the vertebrate eye. Its colour is various and often corresponds with the colour of the insect's skin (white, yellow-white, grey, yellow-grey, and so on to deepest black), and it may even possess a metallic lustre as in fishes, amphibia, reptiles. Even in the simple insect eye there is also a choroid pigment sheathing the optic fibres at the back of the eye, and a separate zone or gurtle representing the iris. And in the simple eyes of many insects and spiders a brilliant tapetum exists!

Nor is there wanting a muscular apparatus in connection with the iris pigment, though the disposition of the muscle-fibre is necessarily different from that of the vertebrate form of iris muscles.

The optic ganglion in many insect eyes is streaked with lines of pigment running from the bottom of the eye through the solid interior, and extending to the zone of pigment lying in the plane of the crystal bodies. Leydig states that this appearance is due to the presence of muscle fibres encrusted with pigment (Dytiscus marginalis). These muscles surround the separate nerve fibres, accompanying them as far as the crystal bodies in which the nerves terminate. Leydig describes four fibres surrounding the nerve-fibres of Acheronta atropos. Each crystal body (immediately behind the corneal lens) is encircled by a ring of pigment (iris) and the muscle fibres arriving at this 'iris' break into a fine network of fibrils. Leydig states that he has repeatedly seen the fibrillœ contracting in the living eyes of certain Hymenoptera.

In the various orders of insects, various differences of detail are noticeable, and in the simple eyes of larval forms, other peculiarities occur which need not be discussed here. But it sufficiently appears from the investigations of Leydig (of which we have given as complete an abstract as our space permits), that in the presence and organisation of all essential parts of a perfect visual apparatus—an optic ganglion (or retina) with a stratum of percipient nerve-elements, and a dioptric apparatus consisting of corneal lenses, iris pigment and muscle, and a choroid pigment—the insect eye whether simple or compound is constructed on the same principle as the vertebrate eye, and presents a veritable identity of essential organs. Before we proceed to describe the many interesting anatomical details of eye structure in the higher invertebrata which have recently been made out, it will be convenient to notice some observations published by Schultze in the third volume of his 'Archiv. f. Micr. Anat.,' respecting the structure and function of the nerve-ends in the eyes of various articulata. The starting point of Schultze's investigations is taken from his recent discovery of a differentiation of substance of the bacilli (rods and cones) of the vertebrate eye in which the retinal nerve-fibres terminate. Assuming the accuracy of the fact that the refractive substance of the outer half of the rods and cones, is disposed in a pile of discs having alternately a different density and refracting power, and omitting for the present all discussion of the important inferences deduced therefrom, we shall follow the reasoning of this acute anatomist in his attempt to apply the fact, as a means of testing the accuracy of Leydig's opinions. This disc structure, says Schultze, must be considered either as a reflecting apparatus and therefore simply subserving some catoptric function in the sense indicated by Brucke, or as nerve-substance so modified in character as to reflect, and at the same time, perceive undulations of light (as suggested by Leydig, see *antè*). If the vision of vertebrate animals be supposed to depend on the sensation of light, or, at least, of colour, effected by this apparatus, the hypothesis must be extended over the visual act and visual organs of invertebrata. Now, as we have already seen, there exists great difference of opinion respecting the nature and properties of the crystal bodies of the eye of articulata; some physiologists considering them to be crystalline lenses, while Leydig maintains that they, together with the club-shaped masses behind them, are modified *nerve*-ends, and as such are to be considered analogues of the outer halves of the cones and rods of the vertebrate retina, in which a conversion of light stimulus into nerve excitation is believed to take place. Consequently, says Schultze, we should expect to find a disc structure in the crystal bodies. Schultze's examination, however, failed to detect any structure of the kind. But *behind* these crystal bodies, namely, in the ribbed double prisms described by Gottsche in the eye of the river crabs, and by Leydig in various orthropode eyes, a very distinct striation of the substance is visible. The substance itself swells up and bends, and twists into curves, and is marked with cross lines just as the substance of the large retinal rods of naked amphibia do when treated with acetic acid or soaked in water. Schultze declares the cause of this striated appearance in the prismatic rods of the crab's eye to be a beautifully distinct arrangement of the substance in discs or plates Each of the strands of which the rod is composed is separately and independently striated, and consists of discs ·003 millimètres thick, each alternate disc being coloured red. In the length of the rod, about twenty discs may be counted. Under high powers, an exceedingly fine secondary striation of the red-coloured discs becomes apparent. In marine species of crab (Palæmon serratus, Carcinus mænus, and Pagurus), the discs are colourless, and their number much greater than in the river crab, but the discs are thinner and exhibit no secondary striation. The rod is buried in a sheath of pigment which adheres more closely to the alternate discs, so that the prepared rod shows alternate dark and light bands.

From Schultze's researches, therefore, it would appear that the club-ended or prism-shaped rod of the crab's eye is really a continuation of the nerve-fibre which, however, according to this anatomist's observations, is separated by a sharply and distinctly marked line of demarcation from the crystal body. This latter element rests with its four-knobbed end in apposition with the rounded and swelled nerve-end so as to embrace it as it were with its four-pointed extremity without any continuity of substance.

The same disc structure was found by Schultze in nocturnal moths in which also Leydig has seen and figured the appearance of cross striation as well as in other insects (beetles, &c.) The discovery is of great importance, as it offers a direct point for comparison with the vertebrate retinal cones and rods, and establishes the doctrine of a fundamental unity of constructive plan of the visual apparatus and an identical physiological principle of vision in all classes of animals.

Histologically we know nothing more of the eye-spots of the lower invertebrata than that they consist of aggregations of pigment round nerve-ends. Of this kind are the eye-spots of many Annulosa and Echinodermata; perhaps also the single eye-spots which rests immediately on the brain-ganglia of Rotifera and Crustacea (Lynceus, Daphnia, Argulus, Artemia, &c.). The next stage of progress consists of the addition of a refracting body having probably the significance of a lens (Caligus, Cyclops, Euchlanis; the paired eyes of many Rotifera, Tardigrada, Annulosa, &c.). Even the eyes of many molluscs show no further organisation. The amount of vision in such animals is probably a minimum, yet physiologically the modified nerve-end (probably possessing some property of internal refraction), and the pigment amassed around it, may be considered a sufficient apparatus for the extent of vision enjoyed. We can admit a morphologically perfect eye, *only where a retina is present,* which contains elements comparable with the bacilli of the vertebrate retina. Eyes of this kind are found in certain annelids, molluscs, and all arthropoda. Where a membranous retina has been examined, the bacilli have always been found (*e. g.*, Alciope, examined by Krohn; Arachnida, Leydig; Heteropoda, Leuckart and Gegenbauer; Insecta, various anatomists). The remaining portion of retina consists, in molluscs and arthropoda, of granular matter, nuclei, cells, and optic nerve-fibres, disposed in a central mass known as optic ganglion. In the compound eyes of arthropoda, &c., connective tissue forms sheaths round the nerve-bundles on and in which pigment is deposited. The retinal elements of the vertebrate eye are likewise imbedded in connective tissue, whose stronger features are recognised as the radial fibre system and "membranæ limitantes."

The differences in outward form of the vertebrate and invertebrate eye, which have led to misconceptions reaching their climax in the theory of a double type of visual organs, may be summed up under two heads, according as they affect the dioptric apparatus, or the proper sensory organ. The former, consisting of cornea, lens, and vitreous humour—on which the general configuration of the eye depends—undergoes such modifications that at first sight a comparison of their extreme differences appears irreconcilable with the notion of an identical 'modus operandi.' The latter (sensory organ) has, only within the last few years, been so far understood as to suggest any intelligible explanation of its structure, and we are still almost wholly ignorant of the nature and extent of its action. Nevertheless, the progress already made, leaves scarcely a doubt respecting the fundamental unity of physical and physiological principles, on which the organ of vision is constructed throughout the animal kingdom. The extreme variations are, in fact, so connected by intermediate forms that all difficulties of interpretation will probably disappear, when the several modifications obtaining in the eye-structures of invertebrata are fully investigated and compared, regard being paid to the great differences in amount and perfection of the seeing faculty possessed by the several classes of animals.

Assuming that the primary condition of the visual act is the formation of an optical image on a given surface, and that vision itself is effected by a special sensory apparatus whose percipient elements will always be found in immediate contact with the plane on which this optical image is formed, it is obvious that the particular construction of the dioptric apparatus is quite subordinate, provided that an optical image is produced. It is a matter of detail whether the lens be a simple or multiple combination, whether it be entirely composed of corneal substance curved into appropriate shapes, or whether a corneal and crystalline lens exist as separate parts concurring together in their action, or whether, as is the case in the eyes of certain small species of cephalopoda, the crystalline lens be a combination of three lenses; or as in the fish, of two halves cemented together. And similarly the absence or presence of a vitreous humour is, so to speak, subsidiary to the general scheme of construction by which an optical image shall be transferred to the locality of the nerve-apparatus.

Assuming further that the conversion of physical impressions into a faculty of sight requires, in addition to an optic nerve (whose fibres simply transmit impressions), certain specialised elements anatomically continuous with these nerve-fibres, it is obvious that such additions must have identical properties wherever they are found, and that they must also have the same relation of contact with the optical image whose impression it is their function to receive. But again it is a mere matter of detail whether these perceiving or receiving elements are massed together in an insect's optic ganglion, or are spread out in concentric layers as in a mammal retina: as also whether the nerves with their retinal elements are directed backwards to the plane of optical images at the fundus oculi or run forwards from the periphery of an optic ganglion to the plane of images formed immediately behind the cornea.

The physical problem concerns the formation of images: the physiological problem concerns the existence and character of the light-perceiving elements. And the harmonious action of both postulates the immediate contact of the percipient elements with the image to be seen. Whatever modifications, therefore, be found in arrangement of the dioptric organs, we may be certain that they are intimately associated with changes in the structural disposition of the retina or optic ganglion. In the vertebrate eye these modifications are comparatively slight, but it is to be noted that in the invertebrate eye greater contrasts of form and 'type' are found than any which can be observed in comparing the well-developed globular organ of a pulmo-gasteropod or cephalopod with the vertebrate 'type.'

Recurring to our insect eye we shall now endeavour to show that the structure of the eyes of articulata exactly accords with and confirms the assumptions just made. And, firstly, with respect to the crystal body: if it be as Leydig believes, a constituent part of the nerve apparatus, its refracting property is none the less to be taken into account in assigning its function. If, however, it has *not*, as Schultz asserts, the distinctive character of disc-structure which this anatomist regards as essential to the terminal element of a nerve of vision, its globular shape and refractive property leaves no other conclusion as to its function than that it acts as a lens and alters the focal position of the image formed by the corneal lens in front. But further, Leydig, as we have seen, denies the existence of a "vitreous body" in articulata, though its absence is not a distinctive character of the invertebrate eye, as in the higher invertebrates a well-developed vitreous humour is found. The compound eye, which appears to fail in this particular, possesses, however, as many "crystal bodies" as it has corneal facets in front and nerve-rods behind. And if we may hazard a conjecture respecting its function (where no certainty as yet exists), we should be disposed to consider the crystal body as the "corpus vitreum" of the compound eye, and for the following reasons. In the first place its position between a corneal lens and an undoubted nerve-termination corresponds with that of the vitreous humour of other eyes. Secondly, it is difficult to conceive how a single nerve-end placed behind a corneal facet, can take up impressions of an entire image, unless we suppose with Wagner that the four-pointed terminations of the nerve-fibre (or consolidated bundle of fibres ?) which embrace the posterior half of the crystal body receive impressions of an image transmitted through its substance and focussed on its posterior surface. Thus the original hypothesis of Johann Müller, namely, that the corneal facet and the crystal body concur together in function as a compound refracting apparatus, is rehabilitated. And Leydig's interesting observation of a pigment band (iris) and muscle spread over the crystal body of hymenopterous insects (see *ante*), supports the assumption that this organ belongs to the dioptric media. The size of the crystal body corresponds exactly with that of the corneal facet, and the perfection of the combination with its iris-like band of pigment and muscle (of accommodation ?) may well surpass that of our compound microscope lenses in which spherical and chromatic aberrations have to be corrected, in order to produce a well-defined image. According to this view the compound eye is an aggregation of separate and distinct visual organs, each consisting of a complete dioptric apparatus with corresponding nerve or nerves of vision.

Histologically the question lies between the opinion advanced by those who consider the crystal body to be developed from cuticular cells, and the opinion which Leydig has expressed as to the anatomical continuity of the crystal body with the nerve-mass behind it. If Schultze be right, a distinct line of demarcation between nerve and crystal body, together with the demonstration of the absence of disc structure, disproves Leydig's observations and the crystal body must be considered a cuticular development whether we view it as a crystalline lens or vitreous humour. But even accepting Leydig's statement, the remarkable modification of nerve-substance which characterises the bacillar structure of all eyes—and which is especially noticeable in the large club-shaped ends of the insect optic fibres—favours our interpretation of the function of the crystal body as a catoptric rather than light-perceiving organ.

We have in the next place to consider the striking and, at first sight, inexplicable inversion of position of the retinal elements in the vertebrate and invertebrate eye. In the former the retinal bacilli rest against the concave "fundus oculi," the rods and cones being directed backwards towards the internal surface of the choroid pigment layer. In the latter the bacilli are found on the peripheral expansion of the optic ganglion and situate in the front of the eye immediately behind the dioptric media. This inverted position of the percipient elements, instead of involving any essential change in the act of vision, affords on the contrary the strongest possible evidence of identity of type. For it must be borne in mind that the optic ganglion is the homologue of the retina, and that in each case the percipient elements must be in the plane of focal images. In the vertebrate eye the central vitreous humour transfers these images to the retina placed behind it. In the insect eye, images are formed in front and probably on the surface of the crystal body itself, which is embraced by nerve-ends. The interposition of a vitreous humour could only be obstructive in a plan by which a number of small eyes are closely packed together. But if the crystal body be accepted as a vitreous humour or its substitute, the position of the nerve-ends corresponds in a physiological sense with that of the retinal rods, both being situate behind the focal plane. The insect eye looks directly *forwards*, the vertebrate directly *backwards*, but in both cases the bacilli are situated *outside* the retinal additions to the optic nerve, and the "inversion" of position really belongs to the vertebrate retina. Both schemes of construction have the same object: namely the utmost possible expansion of surface on which images may be formed, and a corresponding increase of light-receiving elements. Each scheme also has its particular advantages. In the vertebrate eye a large camera surface receives the picture of external objects, and "mosaic vision," if it exists anywhere, must, as Brücke has shown, result from the mode in which the ends of the closely-packed rods and cones are directed against the internal concave surface of the choroid pigment membrane. In the insect eye again, the multiplication of images is the most prominent fact and the comparison of the compound apparatus should properly be made, not with one but with a number of single eyes, single vision being a problem pertaining to the compound eye, similar to the problem offered by the fact of single vision with the two eyes of a vertebrate. The radial disposition of the insect visual apparatus and its gradual concentration upon the optic trunk has its parallel in the vertebrate eye, and the reduction of multiple into single vision is effected in a much smaller space, this being a necessity for the comparatively minute size of the insect.

In connection with this scheme of construction stands the circumstance, at first perplexing, that the pigment of the invertebrate eye appears massed in front of, as well as behind, the optic fibres. The office of the pigment massed in the optic ganglion, is to isolate the strands of optic fibres which have already received their impression of light through the bacilli in front of the pigment. Consequently this pigment does not obstruct the passage of light, but simply confines and determines its passage through the nerve-fibres. The pigment of the optic ganglion is not to be confounded with that of the choroid tunic surrounding the vertebrate retina. The office of the pigment lining the choroid tunic is a similar one in both kinds of eye: namely, to absorb that portion of light which is superfluous, as also to aid in reflection of light back on the nerve-substance. For in the vertebrate retina the ends of the rods and cones are imbedded in the pigment cells, and even partially sheathed by pigment, as Schultze was enabled to demonstrate by the use of osmic acid in his preparation of these parts. The absorbed light further acts as a stimulus to the capillary circulation of the choroid coat, probably exciting to deposition of the pigment granules.

The insect optic ganglion being massed in the central space of the eyeball, some provision was necessary for isolation of its fibres from contact with each other, except at points where junction of fibres or bundles of fibres takes place—hence the pigment lines and bands. In the series of thin concentric layers of retinal structure of the vertebrate retina, isolation of elements by pigment is not necessary; moreover the occurrence of pigment here would obstruct the passage of light to the percipient rods and cones which by inversion of the nerves are situated behind the optic expansion.

In the eyes of the pulmo-gasteropod and cephalopod a large vitreous humour occupies the chief bulk of the now globular ball. Such eyes approach closely in outward appearance to the type of vertebrate eye, having cornea, iris, lens, and vitreous humour, with external investing choroid and sclerotic. Yet in these eyes the same peculiarity in respect to position of the bacillar elements is observed that we have described in the insect eye: namely, that the nerve-fibres, after forming a composite plexus or ganglion, run to their terminations in bacilli without inversion, that is to say, the bacilli are placed in front of the ganglion, yet behind the posterior surface of the vitreous humour. Thus the position of the bacilli is no longer in the anterior of the eye, being thrown back by the interposition of a large central vitreous humour. The disposition of pigment is also modified to suit the new conditions and action of the dioptric media. The scheme of construction as compared with that of the insect eye on the one hand, and that of the vertebrate eye on the other, may be called a mixed one. But in optical principle as well as in physiological "modus operandi" there is absolutely no essential change, and as regards the anatomical basis there is only a difference of detail.

The exposition which we hope to be able to give of the many important and highly interesting facts connected with the structure and function of the retina in the higher invertebrata, demands more space than we have at present disposal, and touches on more questions of homology and comparative physiology than can be satisfactorily dealt with in an article, which has already extended beyond its proposed limits. Reserving, therefore, our account of the structure of other invertebrate eyes for another occasion, we shall conclude with some remarks upon the researches published by M. Felix Dujardin in the 'Annales des Sciences' (1867), who has endeavoured to determine experimentally the optical values of the corneal apparatus of insects.

M. Dujardin begins by declaring himself on the side of those who maintain the essential unity of the function of vision, and the close resemblance of structure of the visual organs throughout the animal kingdom. Every simple eye, he says, and every complete segment of the compound eye is composed of an apparatus performing the office of a lens and dark chamber in order to throw on the percipient nerve an inverted image of external objects. The insect lens apparatus determines the convergence and crossing of rays by which an inverted image is produced, but certain differences in shape, arrangement, and adaptation for near and distant objects are found in different insects. It may consist of a plano-convex or double convex lens formed of solid corneal thickenings in some insects, with the addition of a crystalline body in other insects: or there may be only a thin cornea with parallel surfaces, but curved like a watch-glass, so that the fluid of the chamber retaining the curve in front refracts sufficiently to form an image. As respects the adaptation for distinct vision of near and distant objects, the refractive index and curves of the media employed serve in all cases where the focal length is invariable, that is to say, where the length of the optic chamber is fixed. But if this length be variable the compressibility and elasticity of a transparent body (miscalled "crystalline") affords the mechanism of accommodation. Lastly, the contractility of the walls of the optic chamber itself answers the same end if only this chamber contains a fluid as in diptera. In the last instance also, the optic nerve (ganglion?) formed by reunion of fibres, of which either all or a part are contractile, aids in effecting the necessary correspondence of the percipient nerve-end with the varying plane of focus.

We may remark in passing, that Dujardin's somewhat obscure observation respecting contractile nerve-fibres is better explained by Leydig's account of the muscle-apparatus accompanying the optic nerve-fibres through the "ganglion opticum," and distributed over the crystal body of hymenoptera (see *antè*).

In M. Dujardin's present memoir an account is given of the lenses of simple eyes of insects and spiders ('stemmata'). The promised continuation of his memoir in which the compound eye is to be considered has not yet appeared.

The focal length of the lenses (which vary in diameter from $\frac{1}{123}$th to $\frac{1}{12}$th mm.) was first determined by suitable means, and the author found that some insect lenses gave distinct images at a focal distance easily determined—such insect lenses, namely, as are formed with the aqueous humour by the thin corneæ of diptera and certain lepidoptera, or the plano-convex lenses of coleoptera, which have also a crystal body. But many others, and notably those of hymenoptera and orthoptera, as also those of the stemmata of spiders presented an anomaly. M. Dujardin found, that instead of having a single principal focus for parallel rays, as a spherical lens has, these lenses have as many foci as there are separate concentric zones in the thickness of the cornea, each of these zones having its corresponding focus in which a distinct image is formed; so that the rays proceeding from objects at various distances may be focussed on the retina (or nerve bacillum) by meeting some zone in the corneal substance capable of bringing the object into focus.

Thus a lens may be conceived to be formed of concentric zones whose radius of curvature shortens as the zones in the substance of the cornea successively approach its exterior face. This is we believe the actual fact in the case of the vertebrate crystalline lens, whose inner planes formed by the fibres of the lens have successively longer radii of curvature. Supposing, then, the percipient surface to be a fixed plane, a cornea thus constructed might throw on this surface distinct images of objects, at various distances, by means of its constituent zones, according to their particular curve.

But as it might be supposed that a confusion of images would result, and the image produced by each separate zone be obscured, M. Dujardin determined experimentally that the image given by each zone preserves sufficient definition without being influenced by the rays traversing other zones.

Having caused the curved surface of a plano-convex lens to be ground in zones or concentric rings whose radii of curvature were respectively 96, 82, 68, and 54 mm. (the zones of quicker curve lying successively nearer to the axis of the lens), he applied the lens thus constructed as an objective of a microscope, and found that, besides its principal focus (205 mm.) which was produced by convergence of the axial rays, three additional foci (at 162—140 and 98 mm.) were obtained by convergence of light through three rings of the peripheral surface, each ring being situated in regular order further from the central portion. By means of this objective, and by proper adjustment of the eye-piece, the same distant object may be seen magnified in four different degrees, the image being in each case sufficiently distinct: or by leaving the adjustment of the eye-piece unaltered, and bringing the image always to the same part of the objective, the same object may be seen when removed successively to distances of 21, 36, 67, and 280 centimetres, without any disturbing influence of the rays converged by the other zones on that formed by the zone under examination, whilst the images are confused at intermediate distances. If the zones were more numerous the succession of images would remain unbroken, and vision, therefore, be continuous through space. This, says Dujardin, is precisely what occurs in the simple eyes of arachnida and insects.

The next point which our author believes he has established is that the chemical and optical properties of the corneal lens substance differ from those of the cornea and crystalline lens of vertebrata. The insect corneal substance has no action on polarised light, but exhibits a purple coloration when treated with acid nitrate of mercury.

The morphological differentiation of the corneal substance is not like that of the vertebrate cornea, characterised by remains of cellular structure. The lamellæ of which it is composed are perfectly homogenous, and appear to be continuations of the parallel lamellæ of the integument. Each corneal lamella is thickened in the middle of its posterior surface so as to produce a considerable convexity, and the whole together forms a strong lenticular projection. Leydig, in his various essays, explains the formation of the corneal lens in the same way.

In discussing the corneal lens M. Dujardin objects to the statement of M. Brants, who holds that in the simple and compound eye alike, each nerve fibril plays the same part in the perception of an image. The corneal lens of the simple eye, according to Brants, transmits an image or portion of an image to each *diverging* fibre: that is to say, the impression is supposed to be transmitted in the same line as the axis of divergent rays. On the contrary, in the compound eye, the direction of the nerve-fibre is *converging*, that is to say, the light is transmitted in radial lines from the periphery of the optic ganglion back to the optic trunk. M. Brants' notion of diverging fibres is not borne out by anatomical fact. In the simple, as in the compound, eye the line of direction followed by the minute pear-shaped bodies with their stalks (constituting the bacillar stratum) is a convergent one. And there are no other elements found immediately behind the corneal lens but these; the nerve-fibres of the small optic ganglion of the simple eye being situate further back and imbedded in pigment.

M. Dujardin further rejects Müller's theory of the lens function of the crystal body as an element separate from the corneal lens. For he says, we cannot suppose this crystal body exists to produce a third or fourth refraction of luminous rays. Nor can we admit with Brants and Müller, the existence of an isolated spherical lens behind a cornea having parallel surfaces and equal thickness throughout. The crystal body is simply the central thickening of the whole series of laminæ of which the cornea is composed, and which renders it a lenticular body comparable as to its anterior surface with the segment of a sphere, and as to its posterior surface with a paraboloid or involute curve, the two surfaces belonging to one mass.

"We shall not stop," adds M. Dujardin, "to refute the opinion of Müller, who holds that the simple eye is adapted solely for vision of near objects. But it remains to explain how Müller deceived himself as to the facts of a structure so easily verified. In 1829 the microscope was too imperfect for such observations, but the cause of his error may now be comprehended by the whole world (!) The internal portion of the cornea of the simple eye, forming an oblique projection, any section not made through its axis will separate the projecting part, which will then have precisely the appearance of a globular crystalline lens, with its curved front directed towards the posterior surface of the cornea, or the integument contiguous with its margin."

In other "stemmata" where the internal projection is not inclined, it may happen, continues M. Dujardin, that desiccation of the internal layers may give the appearance of a crystalline lens, as for example may be seen in the dried preparation of the scorpion's eye. The whole inner portion tends to detach itself circularly from the border of junction, and by reason of its lamellar structure it isolates itself often from the external resistant layer. And as one of its faces at least preserves its convexity and smoothness, the separated internal part may be taken for a globular crystalline lens. But in comparing several such desiccated stemmata we may see where the separation has occurred in different points, in such wise as to show a variable number of superposed crystal bodies. And again, in making vertical sections of such stemmata, we immediately recognise under the microscope that the whole is but a series of laminæ, all of which are identical in form (as in hymenoptera): and having a thin outline except in the middle portion, which is thickened in order to form the convexity necessary to refraction.

In similar manner, M. Dujardin interprets the appearances in the eyes of certain Cigulæ. And in respect to other insect eyes he says, "The inner part of the refractive body (corneal lens) being more easily destroyed by maceration, &c., one might fancy that what remains is healthy cornea, and that the crystalline lens had been destroyed, if it were not that one always finds near its edge the outer part composed of laminæ, the thicker central part of which has disappeared. The above facts demonstrate the absence of a proper crystalline lens in the stemmata of insects, and also the possibility in these animals of seeing equally well external objects at different distances by means of the curved cornea, and without the need of any change in the optical apparatus to adapt vision to such variations of distance.

M. Dujardin is thus in perfect accordance with Professor Leydig in representing the supposed crystalline lens of the simple eye of insects as nothing more than a corneal thickening. In so far, therefore, J. Muller's comparison of the simple eye of insects with that of the fish (vertebrate type) is incorrect, as no true crystalline lens is found in the simple insect eye. We have also seen that Müller's account of a "corpus vitreum" is contradicted by Leydig, who declares the structure taken for a "corpus vitreum" to be modified nerve, and, in fact, a portion of the bacillar stratum. The lamented decease of M. Dujardin leaves us in ignorance of the results at which he had arrived respecting the nature of the crystalline body found immediately behind the cornea of the compound eye. But the tenor of his last memoir leaves little doubt that he considered this crystalline body to be the homologue of the corneal lens of the simple eye.

In many eyes of insects and crustacea a lenticular thickening of corneal substance behind each facet is readily demonstrated, and has been figured by Leydig (*Herbstia condyliata, Procrustes coriaceus, Dynastes, Schizodactyla,* &c.). But the existence of this lenticular thickening of the inner layers of corneal substance in the compound eye does not disprove the distinct existence of a refractive crystalline substance situate between the cornea and the admitted nerve-fibre. Whether we are to accept the explanation of Müller and Wagner, that this crystal body performs the office of a corpus vitreum, or the doctrine of Leydig, that it is the actual termination of the nerve-fibre itself (known as retinal rod or cone), or, finally, the later account given of it by Schultze, further observation must determine. But it is at least certain that the corneal thickening in the simple and compound eyes of insects, which answers in place and purpose to the crystalline lens of the vertebrate eye, should not be confounded with the crystal body behind it, and concerning the nature of which so much discussion has arisen.

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REVIEW V.

Injuries and Diseases of the Jaws; the Jacksonian Prize Essay of the Royal College of Surgeons of England, 1867. By CHRISTOPHER HEATH, F.R.C.S. Assistant Surgeon to University College Hospital, and Teacher of Operative Surgery in University College, London, &c. With numerous wood-engravings. London: 1868. Pp. 416.

We are indebted to the Jacksonian Prize Essays for some of the best monographs that have been published of late years. If we look through the list of the subjects that have been set, we shall see what excellent service the College of Surgeons has rendered to the profession by using this means of promoting the study of important questions, or by developing new methods of treatment. And if we glance over the names of the successful candidates, we shall have a sufficient guarantee that the prizes have not been earned without much hard work and painstaking labour. It might be invidious to mention names —we might omit some that ought to find a place in the catalogue — but of those who have carried off the Jacksonian Prize not a few will now be found in the foremost rank of surgeons. This is of itself a proof that the fund is faithfully administered, and that the prize is used to promote the true interests of the healing art.

The essay before us is not inferior to its predecessors, and is well calculated to maintain the high reputation which this series has long enjoyed. The injuries and diseases of the jaws are a subject to which Mr. Heath appears to have paid particular attention ever since he was house-surgeon at King's College Hospital, and had the opportunity of observing Sir Wm. Fergusson's practice. A few years ago he published the histories of several cases in which he had performed Esmarch's operation for anchylosis of the jaw, and he has from time to time brought before the societies remarkable examples of tumours of the upper and lower maxilla. So that he was no novice in the subject when he undertook to write for the Jacksonian Prize Essay of 1867. He has, moreover, taken great pains to put himself in communication with those surgeons who have had the largest experience in this class of cases, and from them he has obtained many interesting reports and some valuable preparations. Thus furnished, he has entered on his task, and the result of his labours is the volume before us, which is certainly the most important monograph upon the subject which has been written for many years.

On taking up Mr. Heath's book, we find that he plunges at once *in medias res.* There is no prefatory paragraph to define the limits of the subject, to explain the scope of the work, and to tell us what we may, and what we must not, expect to find within its pages. A few sentences of this kind, either in the preface or at the beginning of the book itself, would have made the opening more graceful and elegant, and would have been an assistance to the reader. To have a general knowledge of the road by which we are going to travel helps us to appreciate the country as we pass through it. As peak after peak rises on the right hand and on the left, we are able to assign to each its proper relations; and when our journey is over, and we gather up the threads of our information, we are able to say that of this district, at any rate, we have obtained a complete and comprehensive idea. And so it is in the vast domain of literature. To know what is the scope of a book, and in what order the different parts of the subject are presented to our view is a thing which is important for the credit of the author, and still more important for the convenience of the reader. We regret, therefore, that Mr. Heath has not devoted a page at the outset of his work to explaining the extent of his subject, and the order in which he was going to treat it. A few words of this kind would have formed an appropriate introduction, and would have made the opening sentences less abrupt than they now are.

But this is a small fault, and it is much more than redeemed by the solid qualities of the book, by the mass of information which the author has brought together, and the practical bearing of his suggestions. The order in which he treats his subject is this:—first, he deals with fractures of the jaw; then he proceeds to consider the inflammatory diseases; after which he passes on to the tumours which are met with in this situation, beginning with the harmless ones and ending with the cancerous. In this way he reviews the whole series of the injuries and diseases of the jaws. In each instance he has brought forward a number of typical cases, which are briefly noted in the text, and more fully detailed in the appendix; and upon these he has founded his remarks. Indeed, the number of cases is so great that it makes the essay rather dry and hard to read. Any one who undertakes to peruse it from beginning to end, as we have done, can scarcely fail to wish that the cases had been fewer, and the remarks more extended. In some places we should have been glad if Mr. Heath had gone more fully into the subject of treatment, and explained at length the palliative measures that may be employed in the incipient stages of the disease, and before the question of an operation is raised at all. No one is better able than he is to describe the minor surgery of such complaints, and it would have added to the value of his work if he had enlarged upon the minutiæ and details of the early treatment. It is upon such trifles that the comfort, if not the cure, of the patient mainly depends, and it is well worth while to refer to them. And when the question of an operation has to be considered, we cannot help thinking that more use might be made of the *écraseur* than has hitherto been the case in this country. In some of the large pendulous tumours, either of the upper or lower jaw, such as some of those which are mentioned in this volume, it might probably be used with advantage, to take away the bulk of the disease, and to reduce it within moderate limits. If the morbid growth were removed in two or three portions, the operation might be less *brilliant* than when it is taken away at a single sitting, and in the space of a few minutes; but brilliance is no essential part of an operation. The safety of the patient is everything; and he would certainly be less likely to die of hæmorrhage or exhaustion if the tumour were taken away by degrees, than when it is all removed at once. Of course it would only be to a few cases of disease of the jaw that operation by the *écraseur* would be at all suitable; but where it is, we are strongly of opinion that it ought not to be left out of consideration. It has lately been found serviceable in dealing with the malignant growths of the tongue, and by its means they can sometimes be removed with but little loss of blood, and while the patient is under the influence of chloroform. The case of tumours of the jaws is not very dissimilar. In both instances, when the knife is used, the hæmorrhage is likely to be profuse, and for this reason it is inexpedient to produce perfect anæsthesia. But in both cases, we believe, the *écraseur* might be applied with advantage.

We are glad to see that Mr. Heath does not agree with those who hold that when a bone is affected with malignant disease it is necessary to remove it at its articulations—that in a case of cancer it is necessary to amputate at the joint above the disease. This is a doctrine which has been advocated by some surgeons; but we believe it rests upon a fallacy, and a mischievous fallacy too, and we are glad to find that our author rejects it. In dealing with the diseases of the jaws, it would be a particularly difficult doctrine to carry out, and it is well that physiology enables us to dispense with it. The tumours of the jaws—particularly the malignant and semi-malignant growths about the antrum—will always be a formidable class of cases for the surgeon, but we have good reason to believe that the best results will be obtained by the method of operation now chiefly in vogue—namely, by removing the growth from within outwards, and, after we have taken away all the morbid tissue that can be reached, applying the actual cautery, or a caustic paste to the internal surfaces. By this means we may hope to remove the disease, and little or nothing but the disease, while at the same time we stimulate the surrounding tissues, and produce in them a healthy action.

The treatment of diseases of the jaws has been very much advanced by the progress which has been made of late years in dentistry. This strikes us very much in reading the volume before us. In remedying many of the diseases of the jaws, the surgeon and the dentist must work together, and the art of the one supplements the art of the other; and, accordingly, some of the best cases and preparations that Mr. Heath has brought forward in illustration of the injuries of the jaws have occurred in the practice of dentists.

The way in which Mr. Heath's work is printed and finished reflects great credit upon the publishers. It is copiously illustrated, and some of the woodcuts are admirable. We have rarely seen any better than those which illustrate the chapter upon cystic disease of the antrum, where the parchment-like bones which enter into the formation of the nasal cavities are rendered with admirable fidelity.

REVIEW VI.

1. *Researches in Obstetrics.* J. MATTHEWS DUNCAN, A.M., M.D., L.R.C.S.E., Lecturer on Midwifery in Surgeons' Hall Medical School, Physician for and Clinical Lecturer on Diseases of Women in the Royal Infirmary, Fellow and Member of Council of the Royal College of Physicians, and of the Royal Society, Honorary Fellow of the Obstetrical Society of London, &c. &c. Edinburgh, 1868. Pp. 467.

2. *On Ovarian Dropsy: its Nature, Diagnosis, and Treatment.* By I. Baker Brown, F.R.C.S. (by Exam.), late Surgeon-Accoucheur to, and Lecturer on Midwifery and Diseases of Women at, and one of the Founders of St. Mary's Hospital. Second edition. London, 1868. Pp. 280.
3. *The Principles and Practice of Obstetrics.* By Gunning S. Bedford, A.M., M.D., Professor of Obstetrics in the University of New York, &c. Fourth edition. New York, 1868. Pp. 763.
4. *A Practical Treatise on the Diseases of Women.* By T. Gaillard Thomas, M.D., Professor of Obstetrics, &c., in the College of Physicians and Surgeons, New York, &c. Philadelphia, 1868. Pp. 625.
5. *Pathological Anatomy of the Female Sexual Organs.* By Julius M. Klob, M.D., Professor at the University of Vienna. Translated from the German by Joseph Kammerer, M.D., Physician to the German Hospital and Dispensary, New York, and Benjamin F. Dawson, M.D., Assistant to the Chair of Obstetrics in the College of Physicians and Surgeons, New York. London and New York, 1868. Pp. 299.
6. *On Diseases peculiar to Women, including Displacements of the Uterus.* By Hugh L. Hodge, M.D., Emeritus Professor of Obstetrics and Diseases of Women and Children in the University of Pennsylvania. Second Edition. Philadelphia, 1868. Pp. 531.
7. *Cancer of the Uterus and other Parts.* By A. Wynn Williams, M.D. Pp. 23.
8. *The Law of Human Increase.* By Nathan Aden, A.M., M.D., Lowell, Mass. New York, 1868. Pp. 58.
9. *A Case of Missed Labour, with Observations.* Pp. 15.
10. *On Laceration of the Vagina in the Course of Labour.* By Alfred A. M'Clintoch, M.D., F.R.C.S. Pp. 30.

Dr. Matthews Duncan has rescued from the vast pile of conglomerate of which periodical publications are composed another series of essays which well deserved a more extended recognition than "a Quarterly" obtains. We wish to call attention to the volume thus formed, but our space will permit of but a short reference to the various topics embraced in it. Dr. Duncan divides his researches in obstetric science into five parts. I. On Statics of Pregnancy. II. The Pelvis studied with a view to Obstetrics. III. On some points in the Physiology and Pathology of Pregnancy and the Puerperal

State. IV. On some topics in Natural and Morbid Parturition. V. Miscellaneous. The volume is illustrated with forty woodcuts. In Part I is discussed the position of the uterus in pregnancy when it has escaped from the pelvis. The chief support Dr. Duncan shows to be the abdominal wall. The axial line of the long diameter of the uterus passes through the centre of the plane of the pelvic brim, and forms an angle of 20° with the horizon when the woman is erect, the plane of the brim being at an angle of 60°. When the woman is supine the brim-plane is at an angle of 30°, and the uterine axis 60°; and when propped up so that she is in a position between that of sitting and recumbency the plane of the brim is horizontal and the axis of the brim vertical. Now this last is the best posture for trying the fœtal ballottement.

The chapters on the position of the fœtus in uterô are written to combat the teaching of Dubois and Sir J. Simpson, who refer the cause either to vital force in the fœtus or to reflex nervous action. Dr. Duncan argues that gravitation will account for the position usually taken, and contends that in the later months the uterus becomes moulded to the fœtus, and though it may maintain the fœtus in the position gravitation has induced, this and not the uterus determined the position. As the result of sundry experiments, made by floating some fœtuses in a solution of salt and water, Dr. Duncan endeavours to show that the centre of gravity is in the centre of the upper third of the body, and from the position of the uterus in the latter third of the term of pregnancy, the fœtus will physically present with the superior extremity downwards. The fact of frequent malpresentation of a dead fœtus may, he thinks, be explained by a change in gravity, both in the fœtus and the liquor amnii; but he admits that ignorance of the time when the fœtus assumed the position it is found in at the time of birth, as well as of the specific gravity of the amniotic fluid, are obstacles in the way to a correct conclusion. Dr. Duncan does not take into consideration the force that the uterus exercises when it contracts on the fœtus both before the act of expulsion commenced, and during its continuance; and the argument that the fœtus is fixed in its position during the last month, is disproved by the facts Dr. Braxton Hicks has made practical use of in his essay on "Bimanual Turning."

We must very briefly allude to the elaborate essay contained in Part II. After considering the os sacrum as forming part of the vault of the pelvis, and showing that its action is neither as a wedge, nor as a keystone of the arch, Dr. Duncan points out that it is to be regarded as a strong transverse beam, on which the whole weight of the body is suspended by the sacro-iliac ligaments from the posterior iliac tuberosities, and thence is carried to the heads of the femurs or cotylo-sacral beams which lie between them. In the chapters following Dr. Duncan details his views of the causation of the regular development of the pelvis, the formation of the rickety and malacosteon pelvis, the action of the pelvic joints in parturition, and suggests that symphyseotomy has been too freely condemned, chiefly because it has been tried in cases that could not be benefited by it, as in extreme pelvic distortion, but that it might sometimes take the place of craniotomy. Supposing, however, that the operation were successful, as far as gaining sufficient room for the child to pass, the danger of pyæmia, or at least of destruction of the joint, together with the uncertainty as to the amount of space that would be gained, outweighs the chance of saving the child.

The chapters on superfœtation, the place of the insertion of the ovum, the uterus after delivery, and the cervix uteri in pregnancy, will repay perusal by the student, especially as certain errors which have been handed down in most text books are there exposed, as to the decidua, placenta, and actual shortening of the cervix, while the scientific reader will find interesting matter in Part IV, "On some topics in Natural and Morbid Parturition." With the assistance of Professor Tait, some experiments on the strength of the membranes of the ovum were made, and the results tabulated, from which the force exercised by the uterus during labour is calculated, and the conclusion drawn, that as the average weight of the adult fœtus exceeds the propelling power exerted in easy labour (about 6 lbs.) in these labours the weight of the child is sufficient to bring it into the world.

Dr. Duncan combats Naegele's dictum as to the obliquity of the fœtal head on its entrance into the pelvis, maintaining that the head enters the brim discally, but passes the outlet in lateral obliquity. The production of inversion of the uterus is attributed by Dr. Duncan to paralysis or inertia of a part or of the whole of the uterus, and introcession of the inverted portion is caused by force acting either from above or from below. "The conditions fitted to produce the result are," says Dr. Duncan, "to be found in the ordinary bearing down effort, or in the absence of the retentive power of the cavity of the abdomen." By this expression is intended, as explained in a subsequent chapter, "those forces which keep the viscera *in situ*, for which the so-called ligaments are insufficient," referring especially to atmospheric pressure; we think more account should be taken of the muscular power of the parietes, so conspicuous by its partial absence in cases where separation of the recti has occurred to a considerable extent, or pendulous belly is caused by muscular atrophy. We are tempted to ask also, how is it that the great force often employed by the accoucheur in kneading the uterus when indisposed to contract does not frequently produce the rare accident of inversion?

The volume concludes with some brief notes on 'Uterine Metrology,' or the measurements of the area and capacity of the unimpregnated and pregnant uterus, on cases of vagina duplex with uterus simplex, and of saccated uterus. A very formidable list of authors quoted (p. 267) is appended, with a well-selected index.

In our remarks on the points most worthy of notice in Mr. Baker Brown's book on 'Ovarian Dropsy,' we may pass over the chapters on the Pathology, Symptoms, and Diagnosis. An analysis of the cases may offer some evidence, though not very strong, because of the small number under examination, on certain points especially connected with the operation of ovariotomy.

Mr. B. Brown discusses other modes of treatment, such as tapping, tapping with pressure, tapping with injection of iodine, formation of a fistulous opening, incomplete excision; but, as the concurrent opinion of all authorities is in favour of complete excision, where it is possible, the other modes being only either palliative, or to be adopted where further operation is impossible from the circumstances of the case, we need not detain our readers with any discussion on these operations. We have thrown into an analytical table the main features of the 111 cases Mr. B. Brown details, and we shall add to this a brief summary of the whole.

No. of case.	Age.	Married.	Singl	Known duration of disease.	Disease or treatment previous to excision.	Date and health at time of operation.	Adhesions.
1	27	M. Preg. 1	...	14 years.	Tapping and pressure three times, at intervals of 2 years.	...	Slight, recent.
2	23	M. Preg. 0	...	2 years.	None.	May, 1852. Good, feels weak.	Firm band in front divided with knife.
3	37	M.	...	9 years.	Tapping 7 times.	July, 1852. Exhausted and feeble.	Extensive, requiring ligatures.
4	30	...	S.	...	Tapped 9 years previously; return of disease 2 years.	March, 1852. Excellent.	None.
5	20	M. Preg. 1	...	15 months.	None.	June, 1852. Good.	Omental; 1 ligature applied.
6	31	...	S.	18 months.	Tapping 11 days before excision.	Sept., 1852.	Unimportant.
7	57	M. Preg. 7	...	1 year.	Tapping and pressure 4 months before excision.	March, 1854.	None.
8	37	M. Preg. 2	...	2 years.	Pressure.	April, 1854.	Numerous, very strong; peritoneum glued to tumour on left side.

Character of cyst and contents.	Treatment of pedicle.	Sequelæ of operation and treatment.	Autopsy.	Recovery.
3 separate cysts.	Double ligature.	Peritonitis; exhaustion; death on 3rd day.		
2 cysts "proliferous;" dark brown fluid, with scales of cholesterine.	Double Ligature; ends brought out at wound.	Sickness; restlessness; sudden collapse; and death on 2nd day.	Hæmorrhage from band of adhesion.	
Numerous small cysts connected with larger.	Pedicle "tied."	Opium repeatedly; constant nausea and vomiting; shock and exhaustion causing death on 5th day.	Sero-purulent effusion in pelvis, and viscera coated with lymph.	
One cyst; clear fluid.	As No. 2.	Some vomiting and nausea; opium p. r. n.	...	Convalescent on 31st day. Three pregnancies in after life.
Gelatinous fluid, solid.	Three ligatures, &c., as No. 2.	Pain; distension of abdomen; calomel and opium, V.-S. ad ℥xxiv, leeches; vomiting. On 5th day mutton chop. On 15th day wound not healed; appetite bad. 21st day, menorrhagia; exhaustion. Death on 26th day.	Viscera matted with lymph; effusion of pus patches of ulceration on intestines.	
Numerous cysts.	Pedicle tied in 3 portions, and retained external to wound by a director passed through the ligature.	Tympanitis; V.-S. ad ℥xij, opium gr. viii, in 24 hours. Death in 32 hours.		
A thick walled cyst.	Pedicle tied with a double ligature, &c., as No. 6.	Opii gr. ij statim et gr. j o. 2dâ horâ; at night symptoms of peritonitis; V.-S. ad ℥xvj, calomel gr. x, opii, gr. ij.		Convalescent on 14th day. Seven years later in good health.
White oily fluid, with fat. 3 lumps of hair.	As No. 6.	Opii gr. xiv, morphiæ gr. iv in 18 hours; no sleep; vomiting. On 7th day better; next day restlessness, collapse, &c. On 9th, death.	Quantity of sanio-purulent matter in pelvic cavity.	

No. of case.	Age.	Married.	Singl.	Known duration of disease.	Disease or treatment previous to excision.	Date and health at time of operation.	Adhesions.
9	30	...	S.	4 years.	Tapping with pressure 3 months.	March, 1856.	None; cyst easily withdrawn.
10	...	M. Preg. 0	...	2 years.	During March and May, 1858, 3 attacks of peritonitis.	October, 1858.	Numerous; easily broken down.
11	26	...	S.	16 months.	......	October, 1858.	......
12	21	...	S.	11 years.	......	Feb., 1859.	Hardly any; three omental torn through.
13	35	M. Preg. 4	...	4 years.	Repeated tappings.	Feb., 1859. Hard drinker.	A few adhesions; strong and thick, requiring ligatures.
14	45	M. Preg. 2	...	5 years.	Repeated tappings.	Feb., 1859.	None about right ovarian cyst. Left ovarian cyst seemed surrounded by a layer of pelvic fascia; 3 points of adhesion divided with écraseur.
15	35	...	S.	2 years.	......	April, 1859.	Very strong to the colon and bladder.
16	27	...	S.	3 years.	Several severe attacks of peritonitis.	May, 1859. Debilitated.	Slight.
17	32	...	S.	4 years.	......	July, 1859.	A few. Bleeding from one band stopped by division with écraseur.

Character of cyst and contents.	Treatment of pedicle.	Sequelæ of operation and treatment.	Autopsy.	Recovery.
......	As No. 2.	Shock; death on 2nd day.	A pint of sero-purulent fluid in pelvic cavity; very little exudation of lymph.	
Multilocular.	Abdominal cavity sponged out. Pedicle not ligatured but retained outside abdomen with callipers.	"Kept under the influence of opium," "and went on very well."	......	Convalescent in 10 days.
Multilocular; walls rotten.	As No. 10.	No unfavorable symptoms.	......	Quite well in 6 weeks.
Multilocular, contents steatomatous, hair and bone, with teeth.	As No. 10, but with clamp.	Collapse, and death in 30 hours.	Uterus connected with cervix by a small impervious band of membrane.	
......	Abdominal cavity sponged out. Pedicle as No. 12.	Opium freely given, vomiting incessant; death on 6th day.	Intestines glued together with recent lymph, liver pale, easily breaking down on pressure.	
Both ovaries multilocular diseases.	Pedicle of right ovary fixed with clamp as No. 12; the pedicle of left was tied, and the ligature fastened to the clamp.	"Steady progression," "clamp removed on 7th day."	......	Convalescent about 14th day.
Highly albuminous fluid. Multilocular.	Short and thick. Secured as No. 10.	Tympanitis; violent peritonitis; death in 52 hours.	Intestines glued together. Peritoneum much injected.	
Multilocular.	Secured as No. 10.	Went on well till 6th day, then diarrhœa, and death.		
Multilocular.	Secured as No. 10.	Vomiting; tympanitis; (?) pyæmia; death on 9th day.		

No. of case.	Age.	Married.	Singl.	Known duration of disease.	Disease or treatment previous to excision.	Date and health at time of operation.	Adhesions.
18	32	M. Preg. 1	...	3 years.	Tapping, March, 1859.	July, 1859.	Firm adhesions; some bleeding and requiring ligatures.
19	25	...	S.	3 months.	Cyst punctured per vaginam; soon refilled.	Dec., 1859.	Easily broken down.
20	18	...	...	9 months.	Peritonitis once.	March, 1860.	Very slight.
21	85	M.	...	Some years.	Repeated tappings.	April, 1860. Unfavorable.	Firm in pelvis.
22	43	M. Preg. 0	...	2 years.	Tapped 7 times.	July, 1860. Bad.	Omental and pelvic very firm; several ligatures necessary.
23	31	M. Preg. 1	...	4 years.	None.	Nov., 1860. Good.	Very strong and wide; divided with écraseur; some ligatured.
24	48	...	S.	10 months.	Treated for liver and dyspnœa.	Dec., 1860.	None.
25	46	...	S.	2 years.	Tapped 6 times.	Dec., 1860.	Several broken down with hand; considerable oozing of blood.
26	48	...	S.	1 year.	......	March, 1861.	......
27	21	...	S.	2 years.	Tapped twice.	Sept., 1861.	None of consequence.
28	19	...	S.	2 years.	None.	October, 1861. Good.	......

Character of cyst and contents.	Treatment of pedicle.	Sequelæ of operation and treatment.	Autopsy.	Recovery.
Multilocular.	As No. 10.	Peritonitis; death in 24 hours.	Considerable effusion and inflammation.	
Multilocular.	As No. 10.	Two hours after operation; pain in abdomen; V.-S. ad ℥ xij, calomel gr. v, opii gr. ij. Went on well, on 11th day again pain, relieved by sudden discharge of pus per vaginam.	......	Convalescence rapid.
Multilocular.	As No. 2.	Opium per rectum.	......	Convalescence
......	As No. 2.	Death from exhaustion on 2nd day.		
Multilocular.	As No. 2.	Death in 22 hours from shock and exhaustion.	No signs of peritonitis.	
Multilocular, thick dark fluid.	True pedicle, as No. 2; an expansion of adhesions secured in callipers.	No unfavorable symptoms. Callipers removed on 3rd day, with ligature.	......	Quite well in 6 weeks.
Multilocular.	As No. 10. Callipers.	......	......	Quite well in 5 weeks.
Multilocular; contents, pure albumen.	As No. 10. Callipers.	Vomiting constant, low peritonitis. Death on 2nd day.	Large quantities of fresh lymph; kidneys traces of pus; several large clots of blood amongst intestines.	
Multilocular of both ovaries.	The right pedicle was secured by clamp as No. 12; the left by ligature, as No. 2.	Some vomiting and nausea; pulse, greatest rapidity 84.	......	Convalescent in 16 days.
Multilocular.	As No. 10. Callipers.	Some peritonitis, easily subued.	......	Quite well in 6 weeks.
Unilocular.	As No. 10. Callipers.	Symptoms of peritonitis, relieved by turpentine fomentations and inhalations.	......	Quite well in a month.

No. of case.	Age.	Married.	Singl.	Known duration of disease.	Disease or treatment previous to excision.	Date and health at time of operation.	Adhesions.
29	46	M.	...	6 years.	Tapped twice.	October, 1861.	One firm band required ligature.
30	23	...	S.	2 years.	None.	October, 1861.	None.
31	50	M. Preg. 6	...	3 years.	......	October, 1861. Not good.	Tumour firmly bound down, several large vessels tied.
32	27	...	S.	1 year.	Tapped twice.	Nov., 1861. Bad.	Numerous adhesions, one required ligature.
33	18	...	S.	4 months.	General health bad.	Nov., 1861.	Numerous and firm.
34	53	M. Preg. 5	...	1 year.	Tapped 5 times; health much reduced.	Dec., 1861.	None.
35	56	M. Preg. 6	...	2½ years.	Tapped 3 times, twice after admission to hospital.	Jan., 1862.	Some in front easily broken down.
36	30	M. Preg. 3	...	2 years.	Peritonitis after last labour. Tapped once.	Jan., 1862.	Numerous strong adhesions, like cartilage; omentum tied and divided.
37	55	M. Preg. 0		2 years.	Tapped twice.	Jan., 1862. Good.	None.
38	49	M. Preg. 6	...	8 weeks.	None.	March, 1862. Good.	None.
39	29	M. Preg. 0	...	3 years.	Tapped twice. Catamenia absent 6 months.	March, 1862.	Numerous in front. Whole omentum adherent; adhesion to small intestine; bands of adhesion, with pelvic fascia, and uterus.

Character of cyst and contents.	Treatment of pedicle.	Sequelæ of operation and treatment.	Autopsy.	Recovery.
Multilocular.	As No. 10. Callipers.	Flatus, relieved by turpentine fomentations and inhalations.	......	Well in 5 weeks.
Single, proliferous.	As No. 10. Callipers.	.. ...	......	Well in 5 weeks.
Multilocular, very solid.	As No. 10. Callipers.	Death in 40 hours from shock.	Partial peritonitis.	
......	As No. 10. Callipers.	No pain. No opium.	......	Well in a month.
Multilocular.	As No. 10. Callipers.	Good recovery.	......	Well in a month.
Multilocular of both ovaries.	As No. 12. Clamp to each pedicle. Fluid in abdominal cavity not interfered with.	Rapid recovery.	......	Well in a month.
Multilocular.	As No. 10. Callipers.	......	......	Well in less than a month.
Multilocular, many of the cysts suppurating.	As No. 10. Callipers.	Low peritonitis; partial recovery; then diarrhœa and discharge of pus per rectum.	Pelvic abscess; wounds of operation quite healed.	
Single, proliferous cyst.	As No. 10. Callipers.	Went on well 5 days; then diarrhœa and death on 8th day.	Nothing to account for death.	
Multilocular.	As No. 10. Callipers.	No pain, no sickness, no opium for 9 days; then sudden depression. Death on 10th day.	General peritonitis; large white, firm clot in left auricle.	
Multilocular.	Broad pedicle enclosed in 4 pairs of callipers.	Progressed favorably; some sickness.	......	Convalescent in 3 weeks.
	Bleeding points of intestine, where it had been adherent, only arrested by fine ligatures.			

No. of case.	Age.	Married.	Singl.	Known duration of disease	Disease or treatment previous to excision.	Date and health at time of operation.	Adhesions.
40	30	M. Preg. 3	...	1 year.	None. Tapped 2 days before operation; ℥j of brown fluid drawn off.	April, 1862. Good.	One large adhesion to omentum; brown fluid in peritoneal cavity.
41	41	M. Preg. 5	...	2 years.	......	May, 1862.	None.
42	33	M. Preg. 0	...	10 months.	May, 1862. Exploratory tapping; peritonitis.	June, 1862.	Adhesions in all directions.
43	33	M. Preg. 2	...	15 years.	Tapped twice.	June, 1862.	No adhesions.
44	38	M. Preg. 1	...	1 year.	......	June, 1862.	Numerous; whole omentum adherent; hæmorrhage checked by cautery.
45	34	...	S.	1 year.	"Much wasted and very weak."	July, 1862.	None.
46	39	...	S.	3 years.	Twelve months ago peritonitis, June, 1862, tapping, followed by peritonitis.	July, 1862.	Extensive and firm adhesions in all directions; intestines and omentum adherent.
47	16	...	S.	3 years.	Emaciated and dying.	July, 1862.	None.
48	40	...	S.	2 years.	Tapped 10 times.	Sept., 1862.	None.
49	28	...	S.	3½ years.	Tapped Dec., 1861, and again June, 1862.	Nov., 1862. Good.	None.

Character of cyst and contents.	Treatment of pedicle.	Sequelæ of operation and treatment.	Autopsy.	Recovery.
Multilocular.	As No. 10. Callipers.	Progressed favorably.	......	Convalescent in a fortnight.
One proliferous cyst.	As No. 10. Callipers.	Not an unfavorable symptom.	......	Convalescent in less than a month. Ovariotomy again, Feb., 1863, and death.
Multilocular fluid very albuminous.	As No. 10. Callipers.	Did not recover shock of operation. Death in 30 hours.	No cause of death found.	
Proliferous cyst; thin green fluid.	Pedicle secured by silver wire cut off short and returned into abdomen.	Next morning acute peritonitis; V.-S. ℥ xvj; violent vomiting and diarrhœa. Death in 26 hours.	Peritoneum injected; no lymph; pedicle imperfectly strangled; a large clot on cut surface.	
Large proliferous cyst.	As No. 10. Callipers.	Progressed favorably.	......	Convalescent in 3 weeks.
Three cysts; one contained pus and cheesy matter.	As No. 10. Callipers.	Did well for a week, then hospital erysipelas, and death.		
Multilocular.	As No. 10. Callipers.	On 7th day pus escaped from wound, and next day a piece of bowel sloughed away and fæces passed; healthy granulations formed and closed wound.	......	Recovery good, though gradual.
Multilocular.	As No. 10. Callipers.	Never rallied. Death in 27 hours.	Nothing abnormal, but weak heart, with thin walls.	
Multilocular.	As No. 10. Callipers.	Good recovery.	......	Quite well in a month.
Multilocular; of left ovary, fluid very albuminous.	As No. 10. Callipers.	No bad symptom for a week, then hæmorrhage from pedicle, requiring ligature.	......	Ultimate recovery.

No. of case.	Age.	Married.	Singl.	Known duration of disease.	Disease or treatment previous to excision.	Date and health at time of operation.	Adhesions.
50	55	M. Prg. 11	...	3 years.	None.	Dec., 1862.	None.
51	51	...	S.	4 months.	None.	Jan., 1863.	Tumour firmly adherent to peritoneum and intestines.
52	36	M. Preg. 2	...	1 year.	Twice tapped. Health much impaired.	March, 1863.	Whole omentum adherent to tumour.
53	31	M. Preg. 5	...	2 years.	Tapped twice.	July, 1863.	Some omental adhesions.
54	20	...	S.	14 months.	Tapped twice.	Aug., 1863.	Several adhesions, but not strong, secured by silver wire.
55	59	...	S.	9 years.	Tapped July, 1863, 7 quarts of thick fluid.	Aug., 1863.	None of consequence.
56	30	...	S.	4 years.	Tapping and pressure 4 years previously; no return of disease until 6 months ago.	Nov., 1863.	None.
57	35	M. Prg. 11	...	16 months.	Twice tapped.	Dec., 1863. Health broken	Very firm, bleeding freely.
58	33	M. Preg. 2	...	18 months.	......	Dec., 1863.	Omentum adherent, tied with silver wire.
59	17	...	S.	2 years.	......	Dec., 1863.	......
60	28	...	S.	9 months.	......	April, 1864.	Extensive parietal adhesions.

Character of cyst and contents.	Treatment of pedicle.	Sequelæ of operation and treatment.	Autopsy.	Recovery.
Multilocular of left ovary.	As No. 10. Callipers.	"Never had a bad symptom."	......	Recovery.
Colloid very friable of right ovary.	As No. 12. Clamp.	Rigors and frequent pain for some time.	......	Ultimate recovery.
Very little fluid.	As No. 10. Callipers.	Never rallied. Death in 15 hours.		
Multilocular of both ovaries.	Right pedicle as No. 10, callipers; left pedicle as No. 2, but one end of ligature cut off, the other brought out at wound.	Not a bad symptom.	......	Recovery.
Multilocular (?) left ovary.	As No. 10. Callipers.	Progressed favorably.	......	Recovery.
Multilocular of left ovary.	As No. 10. Callipers.	Progressed favorably.	......	Recovery.
Unilocular of right ovary.	As No. 10. Callipers.	No bad symptom.	......	Recovery.
Multilocular of both ovaries.	Left pedicle tied with hempen ligature returned into abdomen; right pedicle secured by two clamps and hempen ligature.	Low peritonitis on 3rd day. Death on 4th.	Left pedicle healthy; right sloughing; general peritonitis.	
Left ovary multilocular; right complicated by large quantities of small cysts.	Left as No. 10. Callipers; right secured by hempen ligature, and returned.	......	......	Recovery good.
Multilocular of right ovary.	Pedicle too short for clamp; tied with hempen ligature and returned.	Sickness, low peritonitis. Death on 5th day.	None.	
Multilocular of left ovary.	As No. 10. Callipers.	......	......	Recovery rapid.

No. of case.	Age.	Married.	Singl.	Known duration of disease	Disease or treatment previous to excision.	Date and health at time of operation.	Adhesions.
61	51	M. Preg. 0	...	7 years.	......	April, 1864. Unhealthy appearance.	Adhesions numerous, some required knife.
62	32	M. Preg. 0	...	17 months.	......	June, 1864.	Adhesions very few.
63	29	...	S.	12 months.	......	June, 1864.	None.
64	34	...	S.	3 months.	......	Aug., 1864.	None.
65	49	...	S.	2 years.	......	Aug., 1864.	Very numerous and firm, divided by knife and ligatured.
66	29	...	S.	12 months.	Inflammation of liver 8 months ago.	Nov., 1864. Emaciated.	Numerous and firm in every direction; some secured with double ligature, some by actual cautery.
67	31	...	S.	5 years.	Exploratory needle passed into tumour, followed by phlegmasia dolens in 24 hours, 6 weeks before excision.	Dec., 1864.	Numerous peritoneal adhesions, broken down by hand, actual cautery to omentum.
68	47	M. Preg. 3	...	5 months.	......	Dec., 1864.	Extensive, firm, actual cautery used.
69	22	...	S.	15 months.	Tapped once 6 months ago.	March, 1865. Good.	Strong, general, actual cautery used.
70	45	...	S.	2½ years.	Ill-fed.	March, 1865.	Few, easily broken down.

Character of cyst and contents.	Treatment of pedicle.	Sequelæ of operation and treatment.	Autopsy.	Recovery.
Multilocular of right ovary.	......	Death in 20 hours.	Peritonitis; kidneys diseased.	
Multilocular of left ovary.	As No. 10. Callipers.	In 2 hours peritonitis. Death on 4th day.	Peritonitis.	
Multilocular of left ovary.	As No. 10. Callipers.	......	......	Recovery Rapid.
Multilocular of left ovary; right ovary also diseased.	Pedicles secured by clamps, as No. 12.	Sickness for several days.	......	Recovery.
Multilocular of right ovary.	As No. 10. Callipers.	Sickness and peritonitis. Death on 4th day.	General Peritonitis.	
Multilocular.	As No. 12. Clamp.	Low peritonitis; active stimulant treatment.	......	Convalescent in 5-6 weeks. Death in 2 months later from intestinal obstruction.
Multilocular of right ovary, only containing ½ pint of bloody fluid.	As No. 12. Clamp.	Patient sustained by beef-tea enemata; discharge from wound offensive; clamp removed on 5th day.	......	Recovery in 3 weeks.
Multilocular of left ovary.	Divided by Mr. John Clay's adhesion clamp, and returned into abdomen.	Not an unfavorable symptom.	......	Rapid.
Multilocular of right ovary; left also diseased.	Right pedicle divided with Mr. B. Brown's adhesion clamp; left ovary divided with knife, cautery applied, and then transfixed with double ligature, as No. 2.	......	......	Slow but continuous.
Right ovary.	Pedicle divided on clamp with cautery, and returned into abdomen.	Low peritonitis in 48 hours. Death in 90 hours.	Intestines matted with recent lymph; no hæmorrhage from pedicle.	

No. of case.	Age.	Married.	Singl.	Known duration of disease.	Disease or treatment previous to excision.	Date and health at time of operation.	Adhesions.
71	41	...	S.	10 years.	Tapped a week before operation.	April, 1865.	Slight and superficial.
72	50	...	S.	18 months.	......	May, 1865.	None.
73	51	M. Preg. 8	...	2½ years.	One year ago a cyst ruptured and discharged per rectum; refilled in 2 months.	June, 1865.	Some on right side, divided with red-hot iron.
74	25	...	S.	7 months.	Tapped 7 weeks ago.	......	Cautery applied to 12 patches of adhesions.
75	23	...	S.	6 years.	......	July, 1865. Good.	None.
76	17	...	S.	6 months.	......	July, 1865.	A few in front.
77	30	...	S.	2 years.	......	July, 1865.	None.
78	37	...	S.	2½ years.	Tapped 6 weeks ago.	......	None.
79	35	M. Preg. 6	...	20 months.	Last pregnancy during growth of tumour.	August, 1865.	One band posteriorly divided by cautery.
80	34	M.	...	1 year.	......	......	......
81	40	M. Preg. 0	...	6 years.	......	......	Omentum being adherent was divided and tumour drawn through.
82	33	...	S.	2 years.	......	......	Recent over anterior wall.

Character of cyst and contents.	Treatment of pedicle.	Sequelæ of operation and treatment.	Autopsy.	Recovery.
Tumour of right ovary; left also diseased.	Right pedicle divided by the red-hot iron, and dropped into abdomen; left incised and surface seared.	Not a bad symptom.	......	Recovery.
Proliferous of left ovary.	Pedicle as No. 70.	......	......	Rapid.
Multilocular of right ovary.	Pedicle as No. 70.	......	......	Convalescent in 4 days.
Multilocular of left ovary.	Pedicle as No. 70.	Internal hæmorrhage. Death in 27 hours.	A pint of blood in pelvis from a vessel that could not be tied during operation.	
Multilocular of right.	Pedicle as No. 70.	......	......	Convalescent on 11th day.
Dermoid of left ovary.	Pedicle as No. 70.	Peritonitis next day; V.-S. ad ℥vj, leeches and poultices.	......	Convalescent on 15th day.
Multilocular of both ovaries.	Pedicle as No. 70.	......	......	Convalescent on 25th day.
Multilocular of left ovary.	Pedicle as No. 70.	Some peritonitis and tympanitis; escape of pus from wound.	......	Convalescent on 18th day.
Multilocular of right ovary, eight pints of pus drawn off.	Pedicle as No. 70.	......	......	Good.
Multilocular of left ovary.	Pedicle treated by actual cautery, an artery in it ligatured.	......	......	Good.
Multilocular.	Pedicle as No. 70.	......	......	Good.
Multilocular of right ovary.	Pedicle as No. 70.	Peritonitis threatened; leeches, turpentine, quinine, to cinchonism.	......	Good.

No. of case.	Age.	Married	Singl.	Known duration of disease.	Disease or treatment previous to excision.	Date and health at time of operation.	Adhesions.
83	48	M. Preg. 11	...	3 years.	......	......	Several mesenteric divided by cautery; one vessel tied.
84	30	...	S.	1 year.	Dec. 8, 1865. Paracentesis for diagnosis, followed by peritonitis.	Jan. 9, 1866.	Extensive thin band to mesentery divided by cautery; 3 arteries ligatured.
85	22	M. Preg. 1	...	18 months.	Peritonitis seven months ago.	Jan., 1866.	Two strong bands, posteriorly divided by actual cautery.
86	40	M. Preg. several	...	3 months.	......	Feb., 1866.	A few easily broken down.
87	27	...	S.	3 years.	Tapped 12 months ago.	Feb. 1866.	......
88	53	M. Preg. 3	...	13 months.	......	Feb., 1866.	Universal peritoneal easily broken down, cautery applied to two patches of bleeding points.
89	34	...	S.	2 years.	Peritonitis severe a month ago.	March, 1866.	Parietal; also a large omental adhesion, treated with cautery and ligatures.
90	54	M. Preg. 0	...	(?) 4 years.	Intestinal irregularity.	April, 1866.	......
91	35	M. Preg. 3	...	1 year.	......	April, 1866.	......
92	31	M. Preg. 4	...	......	Tapped 4 years ago.	May, 1866.	......
93	45	...	S.	4 months.	Health good. Treatment nil.	May, 1866.	None.
94	20	M. Preg. 2	...	6 months.	......	May, 1866.	None.

Character of cyst and contents.	Treatment of pedicle.	Sequelæ of operation and treatment.	Autopsy.	Recovery.
Multilocular not attached to pelvic organs.	Attachments divided by actual cautery.	......	......	Good.
Tumour of right ovary, containing fat, hair, and bone.	As No. 70.	......	......	Convalescent in a month.
Unilocular of right ovary, containing dark gelatinous fluid.	As No. 70.	......	......	Perfectly well in a month.
Multilocular of left side.	As No. 70.	......	......	Convalescent in 16 days.
Multilocular of right ovary.	As No. 70.	......	......	Convalescent in 13 days.
Multilocular of right ovary.	As No. 70. Several vessels also ligatured at edge of pedicle.	......	......	Convalescent in a week.
Multilocular of left side.	As No. 70.	Death on 4th day.	None.	
Multilocular of right ovary; left, cystic calcareous degeneration.	As No. 70.	......	......	Convalescent about 10 days.
Multilocular of right side.	As No. 70.	......	......	Convalescent about a fortnight.
Dropsy of left Fallopian tube; ovary also diseased.	As No. 70.	......	......	Convalescence rapid.
Multilocular.	As No. 70.	......	......	Convalescent in 12 days.
Multilocular.	As No. 70.	......	......	Convalescent in 14 days.

No. of case.	Age.	Married.	Singl	Known duration of disease.	Disease or treatment previous to excision.	Date and health at time of operation.	Adhesions.
95	43	M. Preg. 8	...	13 years.	Thirteen years ago enteritis; 10 years ago tapped and pressure applied; in last 3 years tapped 3 times.	June, 1866.	Slight omental adhesion separated by cautery.
96	29	...	S.	10 months.	Tapped a month ago.	......	......
97	36	M. Preg. 7	...	7 years.	Tumour increased rapidly in last 12 months.	June, 1866.	None.
98	47	M. Preg. 4	...	16 months.	......	June, 1866.	......
99	24	...	S.	9 months.	Tapped a month ago.	June, 1866.	General parietal adhesions.
100	55	M. Preg. 10	...	18 months.	Exploring needle used.	July, 1866.	Small omental adhesion seared and 2 arteries tied.
101	17	...	S.	2 years.	......	July, 1866.	Strong adhesion right broad ligament.
102	56	M. Preg. 2	...	......	Tapped twice in last 3 months; now anasarca of lower half of body, and emaciation of upper.	August, 1866.	The adhesions to broad ligament and pedicle gave way, and were ligatured.
103	50	...	S.	2 years.	Emaciation.	Sept., 1866.	Omental adhesion divided by cautery.
104	20	...	S.	6 months.	Exploring needle used.	October, 1866.	A layer of false membrane between peritoneum and cyst, separated by finger and director.

Character of cyst and contents.	Treatment of pedicle.	Sequelæ of operation and treatment.	Autopsy.	Recovery.
Multilocular of left ovary.	As No. 70.	......	......	Convalescent in 2 days.
Proliferous of left ovary.	As No. 70.	No serious symptom.	......	Recovery excellent.
Multilocular of left ovary.	As 71; pedicle long and vascular.	Abscess formed at umbilicus.	......	Recovery gradual.
Multilocular of left ovary.	Pedicle 6 inches broad, pierced by clamp, and each half seared, and the two halves brought together with silver sutures.	......	......	Good.
Multilocular of right ovary.	As 70, and double ligature applied to stump.	......	......	Excellent.
Multilocular of right ovary; also cystic disease of omentum.	As No. 70.	Peritonitis. Death on 6th day.	General peritonitis; no hæmorrhage from stump.	
Multilocular.	As No. 70.	Peritonitis and death next day.	One pint dark-coloured serosity in pelvis, and a few small clots of blood.	
Colloid multilocular of right ovary.	Pedicle was torn away and ligatured.	Opii gr. ij, and beef tea enemata, 4tis horis; next day quinine to cinchonism.	......	Recovery gradual; convalescent in a month.
Multilocular of left ovary.	Pedicle tied in 3 divisions; clamp applied to each of the three portions which were then cut.	Sickness and exhaustion at first.	......	Convalescent in a month.
Multilocular.	Parallel clamp applied.	......	......	Convalescent in 10 days.

No. of case.	Age.	Married.	Singl.	Known duration of disease.	Disease or treatment previous to excision.	Date and health at time of operation.	Adhesions.
105	32	M. Preg. 3	...	3 years.	Tapped 3 times in last 4 months.	October, 1866.	......
106	44	M. Preg. 0	...	2 years.	......	October, 1866.	......
107	28	M. Preg. 1	...	3 years.	......	Dec., 1866. Good.	Extensive adhesions between pedicle and omentum.
108	39	M. Preg. 4	...	10 months.	......	Dec., 1866. Good.	General adhesion to peritoneum in front.
109	...	M. Preg. 0	...	3 years.	......	Dec., 1866.	General and firm adhesions broken down by hand; 1 ligature applied and hot iron to several bleeding points.
110	31	M. Preg. 2	...	1 year.	......	Feb., 1867.	None.
111		M. Preg. 11	...	12 months.	......	Sept., 1867.	One adhesion to the omentum treated with actual cautery and 2 silver wires.

Character of cyst and contents.	Treatment of pedicle.	Sequelæ of operation and treatment.	Autopsy.	Recovery.
Multilocular of right ovary.	As No. 70. Clamp and cautery.	......	......	Convalescent in 3 weeks.
Unilocular of left ovary.	Pedicle so broad and short that the clamp did not include the whole; twine ligature applied and brought out at wound, as No. 2.	......	......	Gradual.
Multilocular of right ovary.	As No. 70 (?).	......	......	Convalescent in 9 days.
Multilocular of right ovary.	Pedicle tied with 2 twine ligatures, as No. 2.	......	......	Convalescent in 14 days.
Multilocular of left ovary.	As No. 70. Clamp and cautery.	Weak. Bed sore formed on back.	......	Convalescent in 3 weeks.
Unilocular of left ovary.	As No. 70.	......	......	Rapid.
......	As No. 70; on removal of clamp an artery spouted which was tied, and ligature cut close.	Not a bad symptom.	......	Good.

And, first, as to the influence of age on the operation. Arranging the ages into quinquenniads, we find that the proportion of recoveries to deaths after the operation, were—

Age.	15 to 20.	21 to 25.	26 to 30.	31 to 35.	36 to 40.	41 to 45.	46 to 50.	51 to 55.	56 to 60.
Recoveries ...	6	6	17	13	9	5	9	7	4
Deaths	3	1	5	11	2	2	4	2	1
Total	9	7	22	24	11	7	13	9	5
Per cent. Recoveries ...	66·6	85·71	77·27	54·16	81·81	71·42	69·23	77·7	80·00
Deaths	33·3	14·28	22·72	45·83	18·18	28·57	30·76	22·3	20·00

Note.—The whole total of these cases is 107. In two of the 111 the ages are not given. In two death was caused by secondary hæmorrhage from imperfect operation.

Mr. Bryant, in his fasciculus on Ovariotomy, details 25 cases which may be thus arranged:—

Age.	15 to 20.	21 to 25.	26 to 30.	31 to 35.	36 to 40.	41 to 45.	46 to 50.	51 to 55.	56 to 60.	61 to 65.
Recoveries	3	3	1	4	1	2	1	2	...	1
Deaths ...	1	2	1	...	...	1	1	...	...	1
Total	4	5	2	4	1	3	2	2	...	2
Per cent. Recoveries	75·00	60·00	50·00	100·00	100·00	166·66	150·00	100·00	...	50·00
Deaths ...	25·00	40·00	50·00	...	...	33·33	50·00	...	...	50·00

Mr. Spencer Wells' experience is as follows:—

Age.	15 to 20.	21 to 25.	26 to 30.	31 to 35.	36 to 40.	41 to 45.	46 to 50.	51 to 55.	56 to 60.	61 to 65.
Recoveries	8	11	8	15	5	11	7	8	4	1
Deaths ...	0	4	7	6	6	4	4	2	1	
Total	8	15	15	21	11	15	11	10	5	1
Per cent. Recoveries	100·00	73·3	53·3	71·42	45·45	73·3	63·63	80·00	80·00	10·000
Deaths ...	...	26·6	46·6	28·58	54·54	26·6	36·36	20·00	20·00	

Adding the results of the experience of these 3 operators we get from 244 cases the following table:

Age.	15 to 20	21 to 25.	26 to 30.	31 to 35.	36 to 40.	41 to 45.	46 to 50.	51 to 55.	56 to 60.	61 to 65.
Recoveries	17	20	26	32	15	18	17	17	8	2
Deaths ...	4	7	13	17	8	7	9	4	2	1
Total	21	27	39	49	23	25	26	21	10	3
Per cent. Recoveries	80·95	74·07	66·6	65·30	65·21	72·00	65·38	80·95	80·00	66·66
Deaths ...	19·05	25·92	33·3	34·70	34·79	28·00	34·61	19·04	20·00	33·33

We see here the mortality per cent. is least during the first quinquenniad, that it rises up to 26, when it becomes pretty stationary until the age of 51, with the marked exception of the quinquenniad 41 to 45, and then falls until the age 60.

The cases here put together are unfortunately too few to generalise from, but it would be of some importance to arrive at a conclusion as to the *âge d' election* for the operation.

The low comparative mortality during the quinquenniad, 15 to 20 and 51 to 55 seem to indicate that in the first period it is expedient to operate without delay, and that during the second, the chances of success are in favour of the patient.

We observe also that the greatest number of cases occurred from 31 to 35.

Comparing the cases where no adhesions were found, or were easily broken down, with those where the adhesions were firm or required operative division, we find in Mr. Baker Brown's report 55 of the former class against 54 of the latter; and of these 55, 44 recovered while 11 died; of the 54, 32 recovered and 22 died. Amongst Mr. Spencer Wells' patients the adhesions were absent or slight in 68, and firm, often requiring division, and sometimes ligaturing, in 46; while of the 68, death occurred in 23 cases; and of the 46, in 16 cases. Adding the figures of both operators together, we have 123 where the adhesions were slight, and 100 where the adhesions were firm; the corresponding mortality being 34 for the former, and 38 for the latter, or 27·64 per cent. against 38 per cent., bearing out—though not so strongly as some have represented—the general opinion of the increased danger caused by adhesions.

Thirdly, as to the influence of tapping on causing adhesions, and therefore the desirableness of not performing that operation as insisted on by Mr. Bryant. This opinion seems hardly borne out by these statistics, for Mr. B. Brown's cases show that in 47, where the tapping had been previously used, in 25 the adhesions were strong, and in 22 they were slight. And Mr. Spencer Wells' cases show that of 57 which had been tapped, in 32 the adhesions were slight, and in 25 firm. Adding the two series together, we get 104 cases of tapping, in 54 of which the adhesions were slight, and in 50 firm.

The pedicle was treated by Mr. B. Brown in various ways. 1. It was retained outside the abdomen by a clamp or callipers. 2. It was tied, brought outside the incision, and supported by a director passed through the ligature and secured across the abdomen by plaster. 3. It was tied by a double ligature, the ends of which were brought out at the wound. 4. The ends of the ligature were cut short, and the pedicle dropped into the abdomen. 5. The pedicle was divided by Mr. John Clay's adhesion clamp, and allowed to drop into the abdomen. 6. The pedicle was divided by B. Brown's adhesion clamp, and dropped into abdomen. 7. The pedicle was divided by the knife, seared with the actual cautery, and dropped into abdomen. 8. The pedicle was divided on a clamp by the actual cautery, and dropped into abdomen.

Of these various modes of procedure, Mr. B. Brown avows his preference for the division of the pedicle by the actual cautery.

Mr. Spencer Wells treated the pedicle,—1. By clamp. 2. By fixing it in the wound by suture or pin. 3. By ligature, the ends being brought out at the wound. 4. By ligature, the ends being cut short, and the whole dropped into abdomen. 5. By dividing the pedicle with the écraseur, and without ligature.

Of these modes, Mr. Spencer Wells prefers the clamp wherever it can be applied.

The causes of death both in Mr. B. Brown's fatal cases and those of Mr. Spencer Wells, fall under one of the heads of Septicæmia, Peritonitis, Exhaustion, Tetanus, Fibrinous clot in pulmonary arteries, Strangulated gut, Cancer.

A translation of Klob's Pathological Anatomy of the female sexual organs has been fairly accomplished by Drs. Kammerer and Dawson, of New York, and we welcome it here as a work of great use to those whose attention is specially directed to these organs. The treatise is a brief and rather dry summary, unencumbered with theories or therapeutics. The language is remarkably free from those forms of expression—provincialisms, we might call them—which are apt to disfigure the writings of our transatlantic brethren, and, in some cases, infect the writings of medical as well as of ordinary authors.

The subject is primarily divided into,—Anomalies of Formation during fetal, infantile and adult life; Anomalies of Nutrition; Puerperal Affections.

Klob considers the vagina to be a development from Müller's filaments, as well as the oviducts and uterus, and not an inversion of the canalis urogenitalis, but yet he allows that it may exist in the absence of the uterus ovaries and tubes: "For," says he, "although these are absent, the external genitals may be perfect, and whenever the external genitals are perfectly developed rudiments of a vagina also exist."

The anomalies in the development of the uterus are explained by the supposition that one or both of the primary halves of the uterus were arrested in growth, and this condition may be found together with perfect ovaries; so that conception may take place, but habitual abortion is the probable result.

The shape of the uterus Klob considers, with Rokitansky, to be moulded according to the submucous connective tissue, which he traces from the vagina to the cervix, and throughout the uterus. He says that an angle of about 165° is situate at the internal orifice of the uterus or juncture of the body with the cervix, and the constancy of this angle depends on the firmness and disposition of the above-named submucous connective tissue, on which also the amount of flexion is dependent. That this flexion is a normal condition has been lately called in question, but we believe that those who do so have not based their denial on anatomical investigations. If it be admitted that the axis of the body of the uterus is parallel with that of the pelvis or has an inclination forwards, it follows that the cervix must be set at an angle, or its position would not be that usually found on vaginal examination, viz. the os externum or os tincæ somewhat directed towards the ostium vaginæ, instead of looking towards the coccyx, as actually happens in anteversion.

The frequency of sterility with marked antiflexion (60 in 275 Mayer) should cause attention to be paid to any menstrual derangement, to the position of the cervix, and to the existence of cervical catarrh.

The most frequent predisposing cause of retroflexion, which is rarer than antiflexion, Klob attributes to atrophy of the submucous connective tissue, especially around the internal orifice in its posterior semicircle; pregnancy, fibroid and other tumours in the posterior wall, posterior adhesions, the result of pelviperitonitis are also causes.

In the chapter on prolapsus uteri Klob does not trace out the various stages, such as retroversion, which we have always found as antecedent, at least, when the prolapse has been gradual; nor do we think him justified in saying that the organ is always hypertrophied, and in its longitudinal diameter especially increased. We have met many cases, especially in the aged, when the whole uterus was found outside the vulva, but the length, by the sound, did not exceed two and a half inches;

and even apparent hypertrophy is often only infiltration into the loose surrounding tissues.

Stricture of the cervix, Klob says, "is more frequently found at the *internal* than the *external* orifice." And again, "A purely mechanical stricture rarely affects the external orifice." Thus he is in direct opposition to those obstetricians who, placing the stricture at the *external* os, practice division of the cervix with the knife or scissors, as the one and only cure for the alleged constriction.

Atresia cervicis occurring in a menstruating female may cause hæmatometia, and eventually hæmatocele, as Beruntz has ably worked out in the chapter on "Menstrual Retention," in his first volume on 'Diseases of Women,' lately published by the Sydenham Society.

Probably the chapter that will prove most generally interesting is that on the diffuse growth of connective tissue in the uterus, causing that enlargement, more or less general, of the organ which is so commonly known under the names fibroid degeneration, "subinvolution," "chronic inflammation," "chronic parenchymatous inflammation," (Farre). Klob calls this condition "proliferation of connective tissue."

> "In consequence" [says he] "of formative irritation, especially when it has existed for a long time, the whole uterine connective tissue sometimes proliferates, either without accompanying increase of the muscular substance; or if this does occur, the connective tissue predominates to such an extent that the muscular substance is comparatively of not much account. The longer the duration of the disease, the more is the mucous connective tissue transformed into the fibrillary variety, accompanied with contraction of tissue. In the first stages the muscular fibres are broader and hypertrophied, but at a later period may be completely lost in the proliferation of connective tissue.[1] The cause must be sought for in habitual hyperæmia; and I cannot concur in that explanation which interprets the process described as chronic inflammation."

Farther on he observes that the pampiniform and utero-vaginal plexuses are often in a varicose condition, and here we doubtless find the cause of that neuralgic state complained of under the words, "backache," "bearing down," "lumbar weight." The same diffuse growth occurs in the so-called hypertrophy of the cervix, and is often accompanied by an inflammatory condition of the follicles, giving rise to the appearance too well known under the much abused title "ulceration of the womb."

Klob divides papillary tumours of the uterus into condylomata, simple or benign papilloma, cancroïd or cauliflower growth

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[1] Might not this be termed cirrhosis of the uterus?

[epithelioma], and the villous form of medullary cancer. He admits that the benign papilloma is apt to assume a cancroïd form; it would seem, therefore, that in practice they may be considered the same, as removal before the tissues are deeply implicated is the only cure for epithelioma, and the prognosis must always be unfavorable.

We must pass over the full pathological details about mucous polypi, fibroïd, cystoïd, vascular tumours, cancroïd and carcinoma tuberculosis, and give a hasty glance at the part on anomalies of nutrition.

Hyperæmia may be active as a sexual afflux or caused by adventitious growths, or may be passive from impeded venous reflux. The primary consequences are enlargement of the organ and proliferation of connective tissue (chronic engorgement), hypersecretion from the mucous membrane and glands, hæmorrhage or inflammation may also result.

Kiwish's division of the causes of metrorrhagia and menorrhagia is adopted.

Genuine hypertrophy or hyperplasia, when all the normal elements are increased in their due relative proportions, is very rare. The opposite condition, or ' atrophy ' may take place as a derangement of puerperal formative action, or as a result of pressure, as from fibroids, and it is the ordinary condition in aged females.

Genuine metritis Klob has never met with. Endometritis he divides into acute and chronic catarrhal, croupy, and membranous. He observes that females who have suffered for a long time from blennorrhœa have a predisposition to the occurrence of placenta prævia, but states no facts to support the assertion. He notes also the frequent complication of chronic catarrhal endometritis with chlorosis, tuberculosis and heart disease.

Membranous dysmenorrhœa Klob would consider as inflammatory in character, but, we think, without sufficient reason. The pains caused during the exfoliation of the hypertrophied mucous membrane are due rather to the expulsion of a foreign body by the *labour*-like contractions of the uterus.

Rupture of the non-gravid uterus takes place when the cavity has been distended by either fluid or solid contents without corresponding hypertrophy of the walls, or from degeneration of the walls, as by ulcerative carcinoma or abscess. The most common causes of rupture of the gravid uterus are deficiency of substance from arrested development and displacements (retroflexion). In several cases of spontaneous rupture Klob has found fatty degeneration of the muscular tissue. It is agreed by all observers that rupture of the puerperal uterus is much more common in multiparæ than primiparæ, and Murphy believes that it never occurs even in contracted pelves except when there is a degeneration of the uterine tissue.

Dr. Bedford's work, which has now arrived at a fourth edition, only claims our notice as being one of the leading text-books in the United States. Though disfigured in many places by sensational stories, in which our author stands forth triumphant after other doctors' misdoings, the practice inculcated is generally agreeable to that of the English school. He leans to the side of those who like Dr. R. Lee hold in great reverence the ancient dictum about meddlesome midwifery, and is more opposed to the use of the forceps in tedious labour than is now generally held to be good practice.[1]

When the placenta is not thrown off quickly after the birth of the child, Dr. Bedford recommends titillation of the os uteri, and makes no mention of the favorable effect that usually follows compression of the uterus by downward pressure on the abdominal walls, which, as Dr. Eastlake[2] shows, is the successful practice of the Dublin school of obstetrics. Nor does Dr. Bedford notice the recent attempt to make anæsthesia a safe accompaniment of the lying-in-chamber by apparatus such as that of Mr. Ellis, designed to insure the smallest amount of chloroform necessary.[3]

We must briefly call attention to two valuable papers by Dr. M'Clintock: one on a case of missed labour worth observation; the other on laceration of the vagina in the course of labour.

The extreme rarity of the former occurrence may be inferred from the fact that Dr. M'Clintock is only able to collect seven cases.

Laceration of the vagina Dr. M'Clintock thinks occurs more frequently than is generally reported, and it has probably been sometimes mistaken for rupture of the neck of the uterus by inaccurate observers.

The rupture here referred to usually happens at the posterior cul de sac, and takes a circular direction. The constitutional effects are less marked than in rupture of the uterus, the prognosis is more favorable, the etiology is obscure, but it is often found that uterine action and obstruction have concurred in the production of the accident.

Thomas's work on the diseases of women is simply a compilation. The surgery, illustrations as well as matter for the text, taken from Sims, the medicine and pathology from various well-known authors. Like all books of the kind its real practical use is limited to giving the student some superficial information, acting as a kind of hand-post pointing out the direction which must be taken to arrive at real knowledge.

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[1] 'On the Treatment of Tedious Labour in the Second Stage,' J. Thornburn, M.D.

[2] 'On the Management of the Third Stage of Labour,' (reprinted from vol. vi, Obstetrical Transactions') Dr. Eastlake, 1865, p. 21.

[3] 'On Safe Anæsthesia in Labour, with a Description of the Fluids and Apparatus,' Mr. Robert Ellis, 1866, pp. 16.

The titles of sundry pamphlets before us will sufficiently explain their contents, and being reprints from periodicals do not call for any extended notice. At the same time we can heartily recommend some of them for re-perusal.

Dr. Meadows on a 'New Mode of treating Cases of Vesico-Vaginal Fistula,' urges from rational grounds, fortified by two cases, that there is no necessity for keeping the patient after the operation: for that a successful event is as well secured when the patient is allowed to go about her ordinary business, the operation having been performed in the usual manner with interrupted silver suture. We note also two papers on malformation of the organs of generation by Dr. William Turner of Edinburgh; and one by Mr. Garland relating a case of dermoïd ovarian tumour escaping per rectum.

Dr. Denis Phelan republishes his observations on the advantages of home attendance on puerperæ as contrasted with the mortality of lying-in hospitals.

In a pamphlet of nearly sixty pages, adapted rather for the pulpit than for the physiological review in which it first appeared, Dr. Nathan Allen of Lowell, Massachusetts, U.S., endeavours to prove that Malthus and Sadler, and philosophers of this type are wholly at fault, and that the law of increase is regulated by the dress and domestic habits of the women of the country, and considers that "the devotion to dress exhibited in France and America has been a powerful agent in changing the organization of women, and has interfered seriously with the great laws of maternity."

A second edition of Dr. Hodge on "Diseases Peculiar to Women" appears this year with a new introductory chapter reviewing the various English books on the subject lately brought out. As is well known, Dr. Hodge believes that all the complaints of women when no organic disease of the affected part is detected, are referable to "irritable uterus," a condition which he insists has no primary relation to inflammation *à la* Bennett and others, but is usually dependent on misplacement of that organ. He tells us that the various neuralgias and conditions of health, mental and bodily, labelled hysteria are to be cured by a well-adjusted uterine pessary, and that instrument is the lever which bears his name. Dr. Hodge does good service in protesting against the barbarous knife and caustic treatment which, even more in America than in our own land, has become the opprobrium obstetricis, and he urges with truth that it is unscientific to call that inflammation of the uterus which would not be recognised as such in any other organ; but he rides his own hobby quite as hard as those whose systems he denounces.

With the exception of the chapter on the shape of and mode of using his pessary, and the instrument has received universal acknowledgment as to its frequent utility, the volume will not add much to the general stock of knowledge. We observe that the material preferred is vulcanite.

Lastly we announce the *début* of a new periodical, the 'American Journal of Obstetrics,' edited by Drs. Noeggerath and Dawson of New York. If the editors succeed in keeping up a supply of such papers as the one on croup, contributed by Dr. Jacobs, the review ought to find a circulation in England as well as in the United States.

Review VII.

1. *On Vaccination: its Value and Alleged Dangers.* A Prize Essay. By Edward Ballard, M.D., &c. 1868. Pp. 391.
2. *A Handbook of Vaccination.* By Edward C. Seaton, M D. 1868. Pp. 488.
3. *Proposal to stamp out Smallpox and other Contagious Diseases.* By Sir J. Y. Simpson, M.D., Bart. 1868. Pp. 17.
4. *Smallpox and Vaccination.* An Essay, by T. Massey Harding, M.R.C.S. Published by the Ladies' Sanitary Association. 1868. Pp. 46.
5. *Vaccination Impartially Reviewed.* By Ferdinand E. Jencken, M.D. 1868. Pp. 28.
6. *Vaccination: its tested effect on Health, Mortality and Population.* An Essay by Charles T. Pearce, M.D. 1868. Pp. 120.

The subject of vaccination has lately occupied so many writers, and so much that is true and false has been stated and re-stated about it, that we in our vocation of reviewers and sifters of medical literature are compelled once more to give it attention and afford it some of our space. We may fairly admit that the subject within the last few years has come to wear a somewhat different aspect from that which it assumed in the first half of the century. In England a law has been passed which enacts that every English child whatever may be the opinion or prejudice of the parents, shall be vaccinated, and that every medical practitioner who vaccinates a child shall render, *nolens volens*, without fee or remuneration, to the State an account of the operation. Both these laws are enforced by penalties, and provision is made for the prosecution of offenders. Again, cases have occurred on the Continent, and have been discussed in one of the first Medical Assemblies in Europe, from which it has seemed that an operation, hitherto thought almost incapable of exerting any injurious effect, may be the means of conveying into the system the seeds of a dangerous and implacable disease. Whether this be so, or in what degree it may be true or false, we shall hereafter inquire, but the facts that the compulsory Vaccination Act has been promulgated, and that these discussions have arisen, at least afford a sufficient warrant for the re-examination of the subject in our pages.

It would certainly in this place be superfluous were we to reiterate in detail the overwhelming evidence which exists of the benefits vaccination has conferred, and is still conferring, as a preventive of smallpox. It will suffice if we recall a few of the most pregnant and convincing facts, which, although they may be familiar to some of our readers, may not be retained in the memory of all, and may be of use to those who have to deal with unbelieving patients. In the first place, it is an undoubted truth that many persons who have been vaccinated have afterwards been made the subjects of the crucial experiment of inoculation with smallpox matter, and have entirely resisted the virus. Dr. Woodville submitted more than three thousand persons to this test, and in all cases variolous inoculation was performed without success. Vaccinated children have sucked the maternal nipple, around which the skin was thickly studded with confluent smallpox pustules, and have escaped unharmed.

Secondly, statistics, whether on a large or small scale, equally prove the value of vaccination. Mr. Cross, of Norwich, in the epidemic smallpox which ravaged that town in 1819, collected an account of 112 families, made up of 603 individuals, into all of which smallpox was accidentally introduced. Of the 603 persons 297 had before passed through smallpox and escaped; 91 were vaccinated and resisted the contagion, with the exception of two who had the disease in a very modified form; whilst of the remaining 215 who were entirely unprotected, 200 took the disease, of whom 46 died. National statistics all point to the same result. Dr. Lettsom calculated that during the last century of every million of population in England 3,000 died annually of the smallpox; whereas during the ten years 1851—1860, the annual average of deaths from that disease was only 221·4 per million. Then there are the often quoted statistics of Denmark, where vaccination was introduced in 1801. During the previous ten years 2,546 persons had died from the smallpox in Copenhagen, but in the nine years after the introduction of vaccination only 158 deaths from that disease occurred; and from 1810 to 1824 not a single death from smallpox took place. The same kind of evidence meets us from every quarter. Wherever vaccination has been introduced and practised on a scale at all commensurate with the population, the mortality from smallpox has been immediately reduced. As examples take the following abstract given by Dr. Ballard from one of Mr. Simon's calculations. "The smallpox mortality in Trieste during the vaccination period has been 75 times less than it was before, in Moravia 21 times less, in Silesia 29 times less, in Westphalia 25 times less, in Berlin 19 times less, in Sweden 13 times less, in lower Austria 7 times less, in Copenhagen 11 times." Such facts and statistics afford unanswerable evidence that the spread of smallpox has been and may be definitely controlled, and the mortality from it marvellously diminished by vaccination.

It must, however, be admitted that vaccination is not yet a complete success. Dr. Jenner's prophecy that smallpox must be finally annihilated has not yet been fulfilled. The operation is looked on with open or secret aversion by many,—it may be prejudiced, but still well meaning and even intelligent persons, —and there is even a reactionary feeling amongst some members of the medical profession, who, whilst acknowledging that vaccination has produced considerable results, suspect it, nevertheless to have been overrated as a prophylactic; and believe that without care in the selection of vaccinifers it may be the means of doing incalculable harm.

The causes, real or imaginary, which have led to these doubts, and a gradual defection from the universal and jubilant feeling of confidence in vaccination which at one time pervaded, almost without exception, the mind medical, we shall now examine.

A question presents itself at the threshold of the inquiry, which, besides its scientific interest, has a practical bearing upon several of the matters at issue. It is, what is the vaccine virus, and what is its relation to smallpox? It is clear that if, as some writers contend, vaccinia is itself identical in nature with variola, it ought to be capable of conferring the immunity which a prior attack of smallpox will confer,—an inference which has been woefully contradicted by experience. It is well known that Jenner believed that smallpox, cowpox, and horsepox were one and the same disease, originating from the same virus, and that a person who had received and gone through cowpox had actually gone through smallpox itself. This view of the relation between the two diseases has been more or less closely followed by a large school of vaccinographers, amongst whom Dr. Seaton is one of the best known and most influential. The fact that several trustworthy experimenters have found that, in the cow, the vaccine disease will occasionally follow the inoculation of the animal with variolous virus, taken from a person suffering from smallpox, must undoubtedly be allowed great weight, as proof that the two diseases are identical. But the present position of this question (although in our opinion the balance of evidence strongly inclines to the side that asserts the possibility of producing vaccinia by the inoculation of the cow with variolous matter) is such that we think some fresh, carefully performed experiments, undertaken and observed not by one man only, but by a committee of competent persons, might yield a real gain to science. It is admitted on all hands that it is a very difficult thing to obtain vaccinia by inoculating a cow with variolous matter. Mr. Ceely and Mr. Badcock, indeed, both assert that they have succeeded in producing vaccinia, and with the crops of lymph thus raised children have been vaccinated, and the supply so generated has been perpetual through many human generations. Mr. Badcock, however, states that when he succeeded in seven experiments he failed in ninety-three. Mr. Marson tried to variolate forty cows in succession and failed completely. Were these the only experiments on record unbelievers might fairly suggest that the supposed successes were mere coincidences, as the only philosophical idea of cause and effect which we can form is based upon the notion of at least a certain amount of constancy in sequence. On the other hand, Mr. Ceely in three trials only failed ultimately in one; and amongst Continental observers, Vy of Elbeuf recently, Gassner in 1807, and Theilé, of Kasan, in 1836—the latter after several failures, and by observing many precautions in his experiments—have succeeded in obtaining vaccinia in the cow as a sequence to variolous inoculation. Amongst those who have failed in the same experiment are Coleman, Ring, Dalton, Sacco, Feard, and Bousquet, and in 1865 a Commission consisting of MM. Chauveau, Viennois, and Meynet, appointed by the French Academy, performed fresh experiments, and obtained the following somewhat startling results:

"1. That human smallpox may be inoculated upon the ox and horse with the same certainty of effect as the vaccine virus.

"2. That the effects produced, however, by the inoculation of this kind of virus absolutely differ. In the case of the ox, smallpox virus only produces an eruption of papules so small that unless a person were forewarned of their existence they would pass unperceived. Vaccine virus, on the other hand, produces a true vaccine eruption, with large and well-marked pustules. In the case of the horse, smallpox virus produces, as in the ox, nothing more than a papular eruption, without secretion or crusts; and although this eruption is much more marked than in the ox, it could never be confounded with horse-pox, which is remarkable for the abundance of its secretion and the thickness of its crusts.

"3. Vaccine virus inoculated into animals of the horse and ox species preserves them from the operation of the virus of smallpox.

"4. Smallpox virus inoculated into these same animals imparts an immunity from the operation of vaccine.

"5. Although the virus of smallpox be assiduously transmitted through a series of oxen or a series of horses, nothing like vaccinia is developed on either."

Now we agree with Dr. Ballard, that experiments so positive as those of Ceely and Badcock cannot be overturned by negative results. But it is a remarkable fact that the papular eruption following the smallpox inoculation described by the French Commission, seems to have been observed more or less by most experimenters, amongst the rest by Ceely, and the cases in which these papules or tubercles were noticed were by him regarded as failures. The question at issue seems to us to be whether the papular eruption which follows the inoculation of the cow with smallpox matter in a large number of cases is the disease produced by the inoculation, or whether the rarer phenomenon of a vaccine vesicle, which occasionally succeeds the variolous inoculation, is the appropriate pathological sequence of that inoculation. The former is the view taken by the French Commission, and it is supported by the fact that M. Chauveau obtained an ichor from the papules produced on the cow, which being introduced by inoculation into the human system produced smallpox. Moreover, some earlier experiments made at the Veterinary School of Berlin, and at Boston, in the United States, support this observation. When, indeed, we remember that experiment has proved that smallpox and cowpox vesicles may exist close together on the human arm, each perfectly distinct from the other, and each capable of yielding a virus which may reproduce its like, it would be most unphilosophical to declare that variolous and vaccine eruption might not co-exist in the case of the cow. At any rate, such an explanation seems quite as philosophical as that advanced by Dr. Seaton, who thinks that the papules are simply evidences of local inflammation produced by the introduction of the virus which remains unaltered and unabsorbed at the point where it was introduced. He thus—forcibly at least—expresses his theory:

"It is not in the least improbable that if Mr. Ceely or MM. Le Blanc and De Paul had, in the cases they describe, dealt with the tumid papules that arose as M. Chauveau and his colleagues did, they might have got from them the same stuff (*sic*) they had put in—stuff which had undergone no sort of transformation whatever, but which had lain where it was put, as in a pouch, quite inert, giving rise only to local irritation, without inducing any sort of general affection or disease."—P. 58.

This assumption, which appears to us opposed by the best established doctrines of physiology, will hardly, we should think, find general acceptance. Still we are far from denying, or even doubting, that the vaccine disease may follow the introduction of the variolous matter into the system of the cow; we only repeat that the subject has been reopened by the experiments of the French Commission; and for its final settlement we should be glad to see a series of experiments performed on a large scale by a properly qualified committee of observers. The Royal Medical and Chirurgical Society would confer a benefit on science by undertaking such an inquiry. Allowing, then, as Mr. Ceely's and Theilé's positive observations demand, that the inoculation of variola in the cow is followed by the appearance of vaccinia, is this a proof that the two diseases are identical? We answer with Dr. Ballard, Certainly not! If they were they would present identical symptoms. It is only by their symptoms that diseases can be recognised. Again, are they different diseases arising in different animals from the same virus? Again, no! for if this were true, variola being the human disease, and vaccinia the bovine, vaccination would produce smallpox in man, as variola is stated to produce vaccinia in the ox. Dr. Ballard, adopting Dr. Beales' theory of the propagation of disease by living or germinal matter—which when introduced into the system is nourished and reproduces itself by the assimilation of a pabulum pre-existing in the blood,—offers the following ingenious, if not entirely satisfactory, explanation of the difficulty.

"1st. That smallpox, cowpox, and horsepox, are different diseases, each being natural to man, the bovine species, and the equine species respectively. 2nd. That the viruses of these several diseases differ, being the specific products of the several animals in which these diseases naturally occur. 3rd. That when the virus 'germinal matter' or 'contagion' of human smallpox is inserted into the skin of the cow, it finds there material capable, indeed, of maintaining its life and permitting of its generation, but only in a modified manner, so to speak—a degraded manner. The system or skin of the cow is unfitted for the development of smallpox; it can only, by virtue of its natural constitution, develop cowpox: and as smallpox virus operates upon the same constituents as cowpox virus, cowpox is the disease produced. 4th. That it is the same when the virus of horsepox is inoculated into the skin of the cow. 5th. That *mutatis mutandis*, the same explanation must be given of the effect produced when cowpox matter is inoculated into the skin or injected into the vessels of the horse. Horsepox is the disease which this animal alone can develop, and horsepox is developed. 6th. That the constitution of the higher animal, man, is such that he is capable of developing both his own special 'varioloid disease,' smallpox, and the varioloid disease of the cow, but is incapable of developing the varioloid disease of the horse. Still, the virus of each, when inserted into his system, finds there the material for maintaining its vitality, and permitting of its generation. That the virus of smallpox 'degraded' in the generation it has undergone in the cow does not recover itself again when transferred to the human system. It operates upon the material in the blood that smallpox virus operates upon, and produces thereupon a similar but probably less complete change to that which smallpox virus produces. It is the same with horsepox virus. In either case it is cowpox which is produced; but protection is equally afforded, whether the virus be taken from the cow or from the horse, both against cowpox and against smallpox."—Pp. 33, 34.

We are not called upon to defend the weaker points in this explanation. We quote it because we think it may possibly contain a germ of truth. From a review of the whole subject, we may at least arrive at the conclusion, that whatever may be the close relationship of the three diseases, including variola equina, they are not identical; and granting them a common origin, they are not more nearly related than different species of the same genus. This conclusion will help us to estimate, without prejudice, the real value of vaccination. We have stated something of the diminution of smallpox which it has produced. The next question which must be discussed is, Does vaccination retain its power? Is the vaccine virus deteriorated by transmission through a long series of human beings? On this, again, we find different conclusions arrived at by careful observers, and by persons who are equally acquainted with all that has been written on the subject. Jenner believed that, with due care in the selection of subjects, vaccine lymph undergoes no degeneration. Writing to Mr. Moore in 1816, he says:—

> "You are no stranger, I dare say, to a murmur that is speading through various parts of the empire, excited by what has been supposed a deteriorated state of the vaccine matter. Much has been written upon it in the public journals, and much has been said to me in private correspondence. Medical men are more expert than any others in discovering causes without the fatigue of much thinking; and in the present instance they have all hit upon the wrong one. No great wonder.. They attribute the lessened activity of the matter which may happen to fall into their hands, and its disposition to produce imperfect vesicles, to the great length of time which has elapsed since it was taken from the cow, and consequently to the immense number of human beings through whom it has passed. This is a conjecture, and I can destroy it by facts."

He then proceeds to animadvert upon the carelessness of medical men, who, he says,

> "will take anything they can catch under the mere name of vaccine matter, or from a pustule incorrect in all its genuine characters. . . . . . The matter sent out by the National Vaccine Establishment is much complained of."

He adds:

> "I vaccinate the poor here weekly, and the pustules (vesicles, if you please) are in every respect as perfect and correct in size, shape, colour, state of the lymph, the period of the appearance and disappearance of the areola, its tint, and finally the compact texture of the scab, as they were in the first year of vaccination; and to the best of my knowledge the matter from which they are derived was that taken from a cow about sixteen years ago."

In favour of Dr. Jenner's view, Dr. Seaton quotes the evidence of Mr. Steele, of Liverpool, who has had large experience in vaccination, and who states that the lymph he is now using, was supplied originally by Jenner himself, and that it is now producing the same results as at first. In noticing, also, Mr. Marson's observation, that the cicatrices now produced by the lymph he is using, are not so good as those formerly produced by the lymph of the same stock, Dr. Seaton says he cannot think the observation generally applicable, as it is opposed to his own experience derived from inspecting, during the last eight years, the vaccine cicatrices on the arms of more than a hundred thousand persons.

On the other hand, besides common opinion, there are some experiments and statistics which tend to establish that there is a degeneracy produced by long-continued human transmission, and such degeneracy of an animal poison, foreign to man, is, it must be allowed, only what might have been expected, reasoning from analogy and facts well known in the history of disease. Dr. Ballard, to whom must certainly be awarded the palm for calm reasoning and philosophical analysis of the whole subject, has come to the conclusion that there is evidence that the virus degenerates by successive human generation, even when the utmost care is taken in the selection of the vaccinifer, and in taking lymph from pocks of the proper age. We have not space to recapitulate all the facts on which he establishes this opinion, but we note two or three of them. One is, that lymph immediately derived from the cow, or only two or three human generations old, produces effects which are rarely seen after long transmission. M. Bousquet's observations with the Passy lymph, which was but two removes from the cow, and his comparison of its effects with the lymph then in use at the Académie de Medicine, and the later comparison by M. Steinbrenner of the effects of the Passy lymph after it had been in use for four years, with the effects of other new lymph just obtained from the cow, undoubtedly appear to establish a deterioration. The frequent enlargement of the axillary glands, the larger size and better characters of the vesicles, the more extensive and more phlegmonous areola, and the more frequent occurrence of secondary eruption, have been noticed in every case where a new supply of lymph has been obtained from the cow. In the case of the Passy lymph—

> "At first M. Bousquet, after his usual habit, made three punctures with the new lymph; but he soon had to renounce this practice, as the inflammation was sometimes so great as to affect the whole arm and the axillary glands. He says that M. Gasc has never been able to forget one infant that he vaccinated; the pustules were enormous; the inflammation so violent, that baths, poultices, fomentations, and an antiphlogistic regimen scarcely sufficed to calm it; and when the crusts fell off they left an ulceration which took a long time to cicatrise. 'From this moment,' says M. Bousquet, 'I comprehended for the first time the fears of Jenner, who dreaded inflammation so much that he only made one puncture on each arm,' and even then, as soon as he thought the system to be sufficiently impressed, took measures to arrest the progress of the local affection, and even went so far as to destroy the pustule with caustic.' He adds, 'There is nearly as great a difference between the course of the two eruptions as between variola and varioloid.'"—*Ballard*, p. 220.

Now it will not do to reply to this testimony, as Dr. Seaton does, by saying that the lymph then used at the Académie de Medecine, and with the effects of which those of the Passy lymph were compared, was a degenerated lymph and required changing. No doubt it did; but the effects above described are not those of the ordinary lymph stocks of England. In what percentage of cases do we meet with enlarged axillary glands, enormous pustules, and inflammation so violent that poultices, hot baths, and antiphlogistic regimen are necessary? The fact is, that our best authorities on vaccination are recommending that from four to six pustules be made on the arm, whilst Jenner found that one or two were frequently followed by quite a sufficient amount of inflammation and disturbance to be easily controllable. The belief in the degeneracy of vaccine lymph by human transmission, has become rooted on the Continent, and it is, no doubt, one of the causes that has led to the adoption of animal vaccination. Dr. Ballard states, that vaccination direct from the heifer (the disease being propagated by the inoculation of cattle with vaccine virus), is now fully established as a national method in France. It is used by the Government vaccinators, and lymph "from the cow" is sent into the provinces. It has, moreover, been introduced into Berlin, Brussels, and St. Petersburg. In Naples, animal vaccination has been established for twenty-four years. Now, the evidence of all who have engaged in the practice of animal vaccination, agrees in attributing certain characteristics to vaccination direct from the heifer, or with a virus that has undergone only a few transmissions. The evolution is remarkably deliberate, the pock is very full and firm, the appearance of the areola is comparatively late, the lymph retains its limpidness, there is great incrustation and great delay in the falling of the crust. More general fever is experienced, and engorgement of the lymphatic glands is common. Steinbrenner compared the effects of the Passy lymph after it had been in course of human transmission for four years with some new virus which had only passed through twelve human generations; and although the period during which the Passy lymph had been in use was comparatively so short, yet it was long enough for a difference in the effects of the older and newer virus to be observed. Out of sixty vaccinated with the four-year old lymph engorgement of the axillary glands was observed only in twelve; whereas, out of fourteen vaccinated with the newer lymph, enlargement of these glands occurred in twelve. Desiccation took place on the sixteenth day in the patient vaccinated with the Passy virus, but a day later in the case of the newer virus, and in the latter the fall of the crust was somewhat more delayed. The evidence, therefore, that the nearer we approach to the original source of the disease, the cow, the more active is the virus and the more marked are the local and constitutional results which it is capable of producing seems to us exceedingly strong.

We do not at all dispute the fact that by careful selection of vaccinifers, and of the vesicles from which virus is taken, where vaccination is conducted on a large scale, it may be possible to perpetuate a very tolerably active and potent lymph. But even then there are facts, such as Mr. Marson's experience of the deterioration of vaccine scars, which render it very probable that no amount of care will counterbalance the tendency to deterioration by human transmission.

It may, however, be fairly objected that the above experiments prove it probable that the disease vaccinia is more active in its manifestations when animal vaccination or a frequent recurrence to the cow is adopted, but that the mere external phenomena of the disease prove nothing with regard to its anti-variolous power. What is required, is proof that human transmission renders vaccinia a less certain preventive of smallpox. This evidence is certainly very difficult to obtain. There is, no doubt, a general impression that such is the case, but the increased and increasing proportion of the population who are now vaccinated, renders it impossible to obtain accurate proofs. Tried, however, by the test of re-vaccination, there does exist a certain amount of evidence that lymph which has undergone a long human transmission has lost some of its prophylactic influence. Re-vaccination more frequently succeeds than formerly. Of this the statistics of re-vaccination in the Prussian army furnish proof. In Prussia re-vaccination was begun to be practised in the army in 1833, when 33 per cent. of the vaccinated again took the disease.

> "Now" [to quote Mr. Simon], "the annual percentages of successful results for the whole time during which re-vaccination has been practised in the Prussian army, beginning with that number, run thus:—33, 39, 42, 46, 49, 50, 51, 54, 57, 58, 57, 57, 58, 60, 64, 64, 64, 61, 64, 69, 69, 69, 69, 70. The last proportion of success exceeds the double of that with which the series commenced. Thirty-three per cent. expresses the proportion in which persons vaccinated —say, twenty years previously—had, in 1833, to a certain extent lost the influence of their infantine vaccination; it measures the impermanence of certain impressions produced by the vaccinations of 1813; and that impermanence (such as it was) in the effects of vaccination has increased, almost without exception, year by year, during this quarter of a century; so that the vaccinations of 1836, tested by eventual resusceptibility, to cowpox, were not half so stable as the vaccinations of 1813."—*Simon, quoted by Dr. Ballard,* p. 214.

Mr. Marson published in 1853 observations having a similar bearing. He wrote:—

> "The effect produced by re-vaccination sixteen or seventeen years ago was, with some few exceptions, nothing more than a little irritation, or at most an abortive vesicle with irregular areola. But during the last three or four years I have seen a great many persons on whose arms the vesicles produced by re-vaccination have been quite or nearly perfect, even on those who bore good cicatrices from the first vaccination."—*Marson, quoted by Ballard,* p. 213.

Now, it may be objected that susceptibility to revaccination is not equivalent to susceptibility to variolous contagion; and we admit the objection, as many persons have been revaccinated with success after having passed through variola; but, at least, the progressive statistics of the Prussian army show that the later vaccination was not exactly the same thing as the earlier.

Another argument for the deterioration of the anti-variolous power of vaccine lymph by lengthened human transmission may be drawn from the now established facts, that the extent of protection is in a great degree dependent on the number of vesicles raised; so that a person with but one vesicle is said to be badly protected, whilst one with six is well protected; whereas, the reputation of vaccination was obtained when only single vesicles were produced. One of the most remarkable facts connected with vaccination is that established by Mr. Marson, that the severity and mortality of post-vaccinal smallpox is directly in relation with the number of the vaccinal scars.

These are some of the arguments by which the impression that vaccine virus diminishes in power by human transmission are supported. We think they go far to prove the case; but they are worthless to establish any real diminution in the value of vaccination to the human race. The diminution of mortality from smallpox which has signalised this century is the sole effect of Jenner's discovery; and the trifling mortality which now attends every epidemic of variola, is the best proof that vaccination still gains the victory which the early vaccinator claimed for it. Jenner said that experience only could show how often it would be necessary to recur to the cow. All that the supporters of the deterioration of vaccine virus by human transmission would urge, is, that a more frequent recurrence to the parent stock than has hitherto been thought necessary, should be adopted. The experiments of M. Depaul prove that it is quite possible to keep up a supply of bovine virus by vaccinating the heifer, and we should be glad to hear that his experience had been retested in this country.

We must not dwell on the fact which is unfortunately only too patent, that the wonderful prophylactic power of vaccination diminishes as years go on in a certain proportion of individuals, and that, as a matter of precaution, all authorities unite in recommending re-vaccination. In fact, re-vaccination is now recognised by the law of the land. The eighth clause of the Vaccination Act of 1867 provides that "if the Lords of Her Majesty's Council shall have issued or shall hereafter issue regulations in respect of the re-vaccination of persons who may apply to be re-vaccinated, which such Lords are hereby authorised to do, the Guardians shall pay in respect of every case of successful re-vaccination, performed in conformity with such regulations, under such contracts, or under new contracts entered into after the date hereof, a sum amounting to two thirds of the fee payable upon each case of successful primary vaccination." On what principle the fee of eighteen pence is to be cut down to a shilling, when the trouble and skill of the operator must be the same, we must not stop to inquire. We only notice the clause in this place as an authoritative admission that the prophylactic power of vaccination is not, in a certain proportion of persons, an undiminishing force, and that this is one of the circumstances which accounts for the non-realization of Jenner's hope, and for a certain amount of popular distrust of the operation.

The argument against vaccination derived from the alleged increase of deaths from other diseases since its introduction is scarcely worth answering. In the first place, it is simply not true that scrofula, phthisis, and typhoid fever have increased since its introduction, and in the second, if it were true it would be no argument against its practice. People, no doubt, must die of something, and even admitting that a larger proportion die of those diseases (which, however, statistics disprove), it cannot be disputed that the average tenure of human life has been lengthened by vaccination, and that a most horrible and disgusting disease (which, where it did not kill, often rendered its victim blind or deaf, and generally hideous for the remainder of life) has been well-nigh exterminated. The fact, however, is, that fever and scrofulous diseases, as has been proved by Dr. Greenhow, have remarkably diminished since its introduction. Smallpox, it is well known, is one of the most potent agents for calling into activity the seeds of any constitutional disease that may be latent in the system, and to exterminate it or limit its ravages must produce a beneficial effect on the development of tuberculous and scrofulous diseases.

But we must pass on to the consideration of the argument against vaccination which has obtained the greatest hold of the public mind, and the validity of which demands the most careful examination from the medical profession. The argument is that vaccination is dangerous because it is possible to introduce other diseases and notably syphilis into the system together with the vaccine virus. In the consideration of the subject we shall confine our remarks to the case of syphilis. It will be remembered that in 1856 Mr. Simon addressed the following question to a large number of medical authorities in Great Britain, France, and Germany: "Have you any reason to believe or suspect that lymph from a true Jennerian vesicle has ever been a vehicle of syphilitic, scrofulous, or other constitutional infection to the vaccinated person?" A large majority of his respondents, amongst whom were Paget, West, Marson, Tomkins of the National Vaccine Establishment, Acton, Lee, Langston Parker, Chomel, Bright, Watson, Brodie, Joseph Henry Green, and a host of other great names, supported the impossibility of such an occurrence, or at least agreed that they had never seen anything of the kind. On the other hand, here and there, an adverse opinion was obtained. Thus Mr. Ackerley of Liverpool wrote—"I have no doubt that syphilis has been communicated from a diseased to a healthy child by means of vaccination." Dr. Barlow and Dr. J. R. Bennett had both suspected it; Dr. Fleming of Glasgow had a suspicion that he had once seen the disease communicated in that way; Dr. Lever said emphatically *yes;* Mr. Startin said that he had seen syphilis inoculated by public vaccinators from unintentional vaccination. Others, such as Whitehead of Manchester, and Martin of Bristol, gave similar general testimony; whilst Professor Laycock answered philosophically, that—"Our knowledge of morbid poisons is too imperfect to speak positively as to what may happen or what may be suspected 'reasonably' or not." But one of the most remarkable and explicit answers was given by Mr. Jonathan Hutchinson, who wrote—"I believe that I have seen four or five instances in which *local* syphilitic affections were induced by vaccination performed under ordinary circumstances, and by duly qualified men. In one or two of these the *constitution* suffered also, as seen by appearance of disease in several parts, but in others it was not perceptibly affected."

There is one thing that must be borne in mind in forming a judgment on the numerous reports of vaccine syphilis that have been and are afloat. It is that syphilis is a disease which may lie latent in the system until it is roused into activity by an exciting cause, and such cause is frequently the occurrence of another disease producing some grave constitutional change. The syphilitic manifestations occurring in such a case will, of course, never be primary, but always constitutional or secondary. Now in the disease vaccinia, as in its relative variola, we have an efficient cause for the rousing of the latent germs of inherited syphilis in the infant constitution. Inherited syphilis is, of course, always manifested by secondary symptoms, and by secondary symptoms alone; and as the contrary is true of freshly acquired syphilis, we need have no doubt as to the occurrence of secondary manifestations *alone* after vaccination being invariably due to inherited taint roused into activity by the new morbid process to which the constitution has been subjected. We agree with Dr. Ballard, who has argued the matter admirably, and whose chain of reasoning we can thoroughly indorse, that this is the true explanation of a large number of cases of so-called vaccinal syphilis. In other words, that it is not the vaccination, but the child's parentage which is at fault.

But a residuum of cases, although hitherto not observed in Great Britain, will not admit of any such explanation. In these the syphilitic manifestations have invariably been a primary sore developed at the point of inoculation, which has, in accordance with the periods of incubation of the two diseases, followed the vaccine vesicle, and been in its turn followed by constitutional symptoms. Take the Lupara series, for instance, of which the following summary, furnished by M. Depaul, is given by Dr. Ballard:—

"At the close of October, 1856, M. Marone obtained some lymph in tubes from Campobasso; it was transparent, but tinged with blood. A large number of infants were vaccinated, and twenty-three of them were infected with syphilis. He gives the names and the ages, which last varied from five to ten months. All these children, as well as their parents, were healthy at the time of the vaccination. The vaccinal eruption proceeded regularly up to the period of desiccation; but then the dry crust began to soften anew, *and when it fell off an ulcer with indurated base was found to occupy its seat.* In other cases the dry crust remained adherent for an unusual length of time, and then fell off. At the expiration of a few days the cicatrix reopened, and a sore was produced, having all the characters of an indurated chancre, and lasted a month or six weeks. In all the subjects indolent, non-suppurating, enlarged glands in the axilla were observed; in all of them, also, towards the middle of January, the general symptoms of syphilis appeared—roseola, papules, pemphigus, mucous patches on the lips, mouth, anus, and genitals, with inguinal and cervical glands enlarged. The nurses were infected, in their turn, with indurated chancres on the breast: and at a later period—that is, after five or eight weeks—they had also general symptoms, roseola, psoriasis, impetigo, mucous patches, &c. In their turn, also, the mothers imparted the disease to the fathers. M. Marone has since learned that the vaccine sent from Campobasso had been furnished by a little girl, who died some time after vaccination from an eruptive affection, the character of which, however, has not been thoroughly determined. Filomena Lattorti, one of the twenty-three infected, served for new vaccinations. Eleven infants were contaminated. First they had the characteristic primary affection, and then the consecutive phenomena. Mothers were again contaminated, and they infected others. The disease having been at last recognised, a specific treatment was instituted; but, nevertheless, several of the children died. Eleven nurses, infected by the vaccinated children, infected, in their turn, other children who were not vaccinated, but who had been suckled by them. Several of the contaminated women, too, having fallen pregnant, were confined either prematurely, or at full term, of children, dead or living, but in every case bearing the marks of congenital syphilis."—Pp. 333-6.

Such is the history of one of those terrible accidents of which Continental experience affords four or five examples, but of which we have no knowledge in England. It may be a question whether the syphilitic virus was conveyed intermingled with the vaccine virus or through contamination with the blood of the syphilitic vaccinifer; though the present state of our knowledge of the contents of the true Jennerian vesicle, and the existing evidence of the possibility of syphilis being conveyed through the medium of blood certainly inclines us to the latter hypothesis. But it is undoubtedly true that the ordinary care with which vaccination has hitherto been performed amongst ourselves has sufficed to shield us from such appalling results. The fact, however, that in this as in other of the Italian cases local symptoms, in other words chancres, developed at the spots where vaccination was performed, have succeeded the vaccine vesicle and been followed by indurated glands, eruptions, and the other symptoms of constitutional syphilis, leaves no doubt that it is possible to inoculate syphilis with vaccinia.

Whilst we hold it unphilosophical to ignore or attempt to sneer down a fact which has as much evidence as most other facts in medicine; and even more unphilosophical to pretend to refer the Rivalta and other like series of cases to limited epidemics of syphilitic disease arising coincidently, but independently of the vaccination, which seemed to propagate it; we think that the only moral to be drawn from the Italian cases is, that it behoves all vaccinators not to take vaccine matter for use indiscriminately. Careful selection of vaccinifers, and the avoidance, as far as possible, of any intermixture of the vaccine matter with blood, are absolutely necessary on moral as well as on pathological grounds; but when these precautions are put in practice we need not fear vaccino-syphilis. Scientifically the occurrence of vaccino-syphilis is a possibility, but practically, if due care be taken in the taking the lymph and the selection of the child, the possibility fades away to vanishing-point, at least, in England, where inherited syphilis is exceptional amongst the children of married persons.

Thus the possibility of the introduction of other diseases than the vaccine, together with the probability that the vaccine virus itself may become weakened by continued human transmission, especially when no care is taken to choose the subjects, or the vesicles from which the disease is to be propagated, afford arguments for the more careful practice of vaccination, and for all town and district vaccinations being practised on as large a scale as practicable, in order that a large number of vaccinifers may be secured for the purpose of selection. There is no doubt that these desirable ends may be secured by the provisions of the New Vaccination Act, which empowers the Poor Law Board to order the consolidation or alteration of vaccination districts, and directs that each district be placed under one public vaccinator, who, in addition to being a registered medical practitioner, must possess the special qualification in vaccination prescribed by the Lords of the Privy Council. Moreover, in sparsely populated rural districts provision is made that public vaccination be only performed weekly at certain seasons of the year, so that a fair attendance of children may be secured. It has been avowedly the object of the promoters of the measure to throw the public vaccination of London and large towns into the hands of district vaccinators who shall give the whole of their time to its performance. In the country, public vaccination will remain as heretofore, a means of eking out the scanty salaries of the district medical officers.

Of course it may be urged with truth that compulsory vaccination is contrary to the spirit of English legislation, and infringes on the rights of the subject. But we cannot admit the validity of such theoretical objections in the face of the enormous benefit which vaccination well performed is capable of conferring upon a population open on all sides to the introduction of smallpox. The principle invaded by the Compulsory Vaccination Bill is, to a certain extent, although in a much more limited degree, infringed by the provisions of the Contagious Diseases Act. When smallpox has disappeared from the face of the earth vaccination may be dispensed with. Both are evils, but the evil of the one compared with that of the other is so infinitesimal that by comparison it assumes the positive form of an enormous blessing.

Approving, however, as we do, of the main intention of the Compulsory Vaccination Act, we by no means give unqualified praise to several of its provisions. The ordinary fee for the performance of an operation which is confessedly of so great importance is miserably small; and the proposal to increase it by gratuities to be given on the recommendation of an inspector, for the best quality of vaccination as tested by the number and size of scars, is sure to lead to a large amount of ill feeling and discontent. Moreover, we think that the benefit of public gratuitous vaccination should not be extended indiscriminately to all classes of society. A sufficiently large number of vaccinations for all purposes would be obtained by limiting gratuitous vaccination to the children of certain classes. One of the surest means of weakening the feeling of self-respect and self-reliance in the upper poorer and lower middle ranks of society is to accustom them as a right to gratuitous medical assistance. This has already been done to a great extent by the multiplication of unnecessary hospitals and dispensaries, and the Compulsory Vaccination Bill is another step in the same downward direction. It is moreover unjust to the medical profession who derive an income from the practice. We do not complain of the extinction of small vaccinating districts or of the reduction of the number of public vaccinators, for we hold that class interests must yield to considerations of public benefit. But with large stations where children for arm to arm vaccination, or at least a supply of fresh vaccine matter, may always be obtained, we maintain that there is no necessity to hold out a temptation to persons who can well afford to pay their own medical man to apply to the public vaccinator. It is equally an injustice to the private medical practitioner whom it robs, and to the poor ratepayer, who is taxed to pay for the vaccination of children of persons better off than himself.

Another blot in the Compulsory Vaccination Act is that it compels the private medical man to furnish the state with a certificate of each vaccination he performs without fee or reward. We need not dwell on the manifest injustice of this. We only express our opinion that the Legislature would not have ventured to lay such a tax upon the time and skill of any other profession, nor would any other profession have submitted so tamely to the injustice.

In conclusion we may express our hope that the medical department of the Privy Council, or of the Poor Law Board, will take means occasionally to renew the supply of vaccine lymph used throughout Great Britain by recurrence to the cow. The practice of animal vaccination is undoubtedly beset with too great difficulties to be adopted on anything like a large scale. But there seems to be no great difficulty in keeping up a supply of bovine lymph by the inoculation of heifers, and we cannot but think that such a supply might with great advantage be maintained at the expense of Government for occasional distribution throughout the country.

We have left ourselves but little space to criticise the various works the titles of which we have placed at the head of our article. Dr. Ballard's book has already received our praise. It is undoubtedly one of the best and most philosophically written monographs on a medical subject with which we are acquainted. Dr. Seaton's book is an excellent one, and especially valuable for its practical teaching. Its positive tone, however, we fear will not recommend it to all professional readers. Sir James Simpson's proposal to stamp out smallpox by isolation is founded on the experience of the rinderpest. Vaccination is, at least, a remedy more easily applicable, and at all events should first be fairly tried. Mr. Harding's, which, like Dr. Ballard's, was one of the works competing for the prize offered by the Ladies' Sanitary Association, is in most points an excellent essay, but the author is, as we think, mistakenly opposed to the principle of compulsory vaccination. Dr. Jencken also writes well in favour of the practice of vaccination. Dr. Pearson's essay is the work of an opponent. He asks for a full and impartial inquiry, and that all laws on vaccination should be suspended for ten years, during which the inquiry is to be carried on. Our answer is, that the experiment has been made in every country in Europe, nay in the civilized world, during the last seventy years, and the result is best expressed by the simple fact noticed by Dr.

Ballard, that in no instance in which the practice has received a national confidence has that confidence ever been withdrawn.

REVIEW VIII.

(*Continued from vol.* xlii, *page* 389.)

1. *Epidemic Meningitis, or Cerebro-spinal Meningitis.* By ALFRED STILLÉ, M.D., Professor of the Theory and Practice of Medicine, University of Pennsylvania, &c. Philadelphia, 1867.
2. *Eighth Report of the Medical Officer of the Privy Council*, 1865. London, 1866.
3. *A Biennial Retrospect of Medicine and Surgery for* 1865-6. (New Sydenham Society, 1867.)
4. *Report of the Proceedings of the Medical Society of the King and Queen's College of Physicians of Ireland.* (In 'Medical Press and Circular,' 1867.)
5. *Klinische Beobachtungen uber Meningitis Cerebro-spinalis epidemica.* Von Prof. ZIEMSSEN und FRIEDRICH HESS, in Erlangen.

Clinical Observations on Epidemic Cerebro-spinal Meningitis. By Professors ZIEMSSEN and FRIEDRICH HESS, of Erlangen. From the 'Deutscher Archiv für Klinische Medicin,' 1866. Bd. I, pp. 72 *et seq.*, and pp. 346 *et seq.* ('German Archives of Clinical Medicine.')

6. *Four Cases of Cerebro-spinal Meningitis in Shorncliffe Camp.* 'Medical Times and Gazette,' April 4th, 1868.
7. *A System of Medicine.* Edited by J. RUSSELL REYNOLDS, M.D., &c. London, 1868. Art. *Epidemic Cerebro-spinal Meningitis.* By J. N. RADCLIFFE.
8. *Zur Pathologie der Epidemischen Meningitis.* Von Dr. KLEBS, in Berlin. 'Virchow's Archiv,' 1865. Band XXXIV, pp. 327 *et seq.*

On the Pathology of Epidemic Meningitis. By Dr. KLEBS, of Berlin.

9. *Publications of the Massachusetts Medical Society.* Vol. II, No. 1. *Spotted Fever, or Cerebro-spinal Meningitis in the State of Massachusetts.* Report, &c. Boston, 1867.

10. *Proceedings of the Pathological Society of London.* Vol. XVIII. 1867.

11. *A Report upon Epidemic Cerebro-spinal Fever.* By ED. W. COLLINS, M.B., &c. 'Dublin Quarterly Journal of Medical Science,' August, 1868.

12. *Contributions relating to the Causation and Prevention of Disease, and to Camp Diseases; together with a Report of the Diseases, &c., among the Prisoners at Andersonville, GA.* Edited by AUSTIN FLINT, M.D. New York: Published for the U.S. Sanitary Commission, by Hurd and Houghton, 459, Broome Street. 1867.

Dr. Murchison has published "A Case of Cerebro-spinal Arachnitis" in a female æt. 38, and adds the following:—

"*Remarks.*—This case is of interest from the great rarity of the lesion met with after death; for in this country meningitis independent of injury, is almost always tubercular. From the so-called "epidemic cerebro-spinal meningitis" of the Continent and America it differed both anatomically and clinically. In the epidemic affection, the exudation had been found to be, for the most part, subarachnoid, and chiefly on the posterior surface of the cord. Hirsch, however, had described cases of the continental disease, in which the cavity of the arachnoid had been found full of purulent fluid. Clinically it differed from the symptoms which had been laid down as essential to the Continental affection in the following particulars:—1. In the invasion being gradual and not sudden; 2. In the absence of vomiting; 3. In the absence of severe headache; 4. In the absence of any spasmodic contraction of the muscles of the neck, causing retraction of the head; 5. In the absence of hyperæsthesia; 6. In the temperature being but slightly elevated. The occurrence of herpes on the lips, however, was interesting, inasmuch as it had been described as a very common appearance in the epidemic meningitis of the Continent.

But on the other hand it may be well to refer to the particulars of a case which was some time ago under my care in the London Fever Hospital. A man, æt. 60, was admitted on October 23rd, 1865, who presented all the so-called pathognomonic symptoms of epidemic cerebro-spinal meningitis, including severe headache, moaning, retraction of the head, rigidity of the arms, and vomiting. After death, about the tenth day of his illness, however, no appreciable lesion of the membranes of the brain or spinal cord could be discovered, and the cause of death was ascertained to have been uræmia from contracted kidneys, and recent pericarditis. It would appear then, that, for the purposes of diagnosis, the symptoms of epidemic cerebro-spinal meningitis have been too rigidly defined.

There have always appeared to me good grounds for doubting the existence of "cerebro-spinal meningitis" as a distinct *epidemic* *specific disease.* At all events there can be no doubt that the lesions of that affection have occurred at certain times as complications of such specific diseases as typhus, yellow fever, and probably also severe remittent fever. Under these circumstances the symptoms and cause of the primary disease would, of course, be materially modified, just as they would be modified by the supervention of such local complications as parotid bubo or congestion of the lungs. To argue that the symptoms of cerebro-spinal meningitis are at complete variance with those of uncomplicated typhus is beside the question. Notwithstanding what has been written to the contrary, I am still inclined to regard *epidemics* of cerebro-spinal meningitis much in the same light that Professor Hebra, of Vienna, regards epidemics of miliary fever.[1] The cerebro-spinal lesion, like the miliary eruption, are secondary to more general disease; although, in the former case, the symptoms of the local affection are so violent and peculiar, as to materially modify, or even eclipse those of the primary affection. This primary disease may vary in different epidemics, and thus account for the widely different views entertained on many points relating to the etiology of "epidemic cerebro-spinal meningitis." Like other local diseases, however, it is probable that cerebro-spinal meningitis also occurs occasionally as a primary idiopathic affection independent of any blood-poison."

We quote Dr. Murchison's remarks *in extensó*, as we think it better that those of our readers who may not have access to his contribution to the 'Pathological Reports,' may be enabled to judge of the matter for themselves. We would ask does Dr. Murchison doubt the existence of such a disorder as *epidemic cholera,* because gastric disorders, at times that it is epidemic, are prone to assume the choleraic form?

Again, he says, that variety in different epidemics may account for the widely different views entertained on many points relating to the etiology of "epidemic cerebro-spinal meningitis." We would ask upon what subject, medical, legal, political, or other, are all men, scientific or unscientific, agreed?

Of this we feel convinced, even if Dr. Murchison's reasoning could shake the result of our own observations, that the collective evidence of the existence of the disorder is amply sufficient; afforded as it is by many of the first men in the profession in Continental Europe, in Ireland, and America, and by Dr. Sanderson and others in England who have had opportunities of witnessing the disease.

Before leaving the consideration of Dr. Murchison's observations we would say that it is not from the slightest desire to dictate as to the exact nature of the disease, or as to its nomenclature, that we have written as above, but because we could not feel justified in endorsing his views as to its epidemic existence. As to its specific nature we think there is much to lead to the opinion, as expressed by Dr. Stokes, that "this disease was a blood-poisoning, and that the cerebro-spinal affection was a secondary disease in this form of fever." And yet the general designation given by so many different schools of medicine in the old and in the new worlds is fully justified by the general character of the disorder, both in life and post-mortem.

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[1] 'Diseases of the Skin,' Syd. Soc. Transl., 1866, vol. i., p. 387.

Mr. Simon mentions in his preface to the important report by Dr. Sanderson, the occurrence of meningitis in successive small epidemics in the temperate zone of Europe and America, and in North Africa; he does not consider it more than slightly if at all communicable from the sick to those in health.

The conditions under which the disease appears are for the most part those generally considered as unhealthy.

The ill-housed, ill-fed, and overcrowded, are those most apt to suffer.

Workhouses, convict prisons, schools, and, above all, barracks, and the worst part of all such buildings, are those in which the disease is most apt to appear.

With the experience among the French troops, and the corroborative evidence from other quarters, it would be unwise to overlook the possibility of the spreading of the disease when due sanitary precautions are not observed.

Bleeding and mercury Simon regards as having little to recommend them. Opium appears of some benefit in certain cases; cold applied locally as first recommended by Esmarck may be of benefit. We must, however, observe, that it proved of little value in some cases in which it was tried in Dublin.

Dr. Sanderson describes the country about the lower Vistula, where meningitis prevailed in 1865 as being malarial, with a very severe winter temperature.

The dwellings of the population for the most part are not provided with sanitary requirements, and the people generally are far more dependent upon poor relief than is the case in England. The drinking water is of rather inferior quality.

Dr. Saunderson gives the following summary descriptive of epidemic meningitis as seen on the Vistula:

"In adults, the disease begins almost invariably with shivering, profuse vomiting, intolerable headache, and giddiness. After these symptoms have continued for several hours the patient's thoughts become confused. The headache continues, while other pains fix themselves in the muscles of the nape of the neck, of the small of the back, or of the abdominal wall. At this part of its progress the malady advances so rapidly that, within a few hours after the appearance of the first symptoms the patient becomes violently delirious, while, at the same time, the head is thrown back, and the thighs are drawn up by muscular contraction. The delirium lasts for a few days only.

"In the worst cases the patient lapses into profound insensibility, which continues until death. In a few rare instances, he regains complete consciousness as the delirium ceases, and enters on convalescence. Much more frequently he is left on the third or fourth day of the disease, if he survives his first onset, in a state of extreme nervous depression, which is usually of long duration. This condition is characterised by impairment of consciousness, rather apparent than real, perversion both of common and special sensibility, marasmus, and excessive muscular weakness.

"During the continuance of the state of depression the patient is liable to frequent recurrence of the initial symptoms.

"Although so profoundly prostrate and indifferent to external impressions that he is incapable of replying to questions, he frequently utters piteous cries of pain. At night he sleeps little, usually wanders quietly, and is often subject to hallucinations. At any moment his life may be imperilled, either by secondary affections of the lungs or other vital organs, or by a recrudescence of the primary disease. The author treats of the evil effects left by the disease, if it do not prove rapidly fatal, and of those cases which though unaccompanied by violent symptoms are attended with sleeplessness, night wandering, &c.; in these latter cases serious symptoms, as tedious and exhausting as those which accompany the advanced stages of severe cases, are apt to arise."

Dr. Sanderson observes,

"In the four cases of adults which terminated fatally in the hospital at Dantzic during my visit, the temperatures observed before death were severally 101·8, 102·4, 102·2, and 101·8. In each the respiration gradually increased in frequency till it attained, in the case of Stroessel, who died on the tenth day, 48; in Rahmell and Dupré, each of whom died on the fifth day, severally 60 and 76. In the case of Boldt, who died on the fourth day, the breathing was not counted, but its acceleration was similar. The corresponding pulse rates in the three cases first referred to were severally 120, 128, and 124. These facts, combined with the evidences of hypostatic hyperæmia observed after death, afforded ground for the inference that in these instances interference of the exudation with the respiratory functions of the medulla oblongata was the immediate cause of death."[1]

In Germany, Sanderson considered that the cold, damp, and malarial position of affected districts had reference to the causation of this disease. Overcrowding, want of ventilation and of house drainage, existed in the dwellings where the disease occurred under this author's observation, as well as in the houses that were unaffected.

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[1] This view is confirmed by observations made in Baden.—Niemeyer, loc. cit., p. 50.

In the spring of the present year (1868), four cases of the disease occurred among the troops in Shorncliffe camp; all were men of the 12th depôt battalion and three were in the same hut. Under the huts the earth was damp and mouldy, and the external apertures were closed with shingle. The writer of the communication from the camp suggests that these circumstances may have only been a coincidence, and that the soldiering age and living in masses may yet probably furnish a clue to its etiology.[1]

Dr. Hudson, of Dublin, in a recent work on fever, records some cases of the disease in which the hygienic conditions were decidedly bad.

Mr. J. N. Radcliffe[2] quotes several authors, showing the tendency of the disease to appear in the colder months, and that it appears on all soils indifferently, and in some epidemics among the affluent as well as the poor, and ill-fed, and ill-housed, and badly clad. He concludes by saying that in certain outbreaks the rich and well-to-do escaped much better than the poor and miserable.

He states that—

> "The history of epidemic cerebro-spinal meningitis dates only from the fourth decennium of the present century. At that period the disease was for the first time clearly distinguished as an independent malady; and with the light then obtained, outbreaks which had occurred earlier in the century, in various localities of both the eastern and western hemispheres, and had been recorded under other names, were recognised as of a similar character. It has been sought, indeed, to show that epidemic cerebro-spinal meningitis has probably existed from remote periods (Tourdes, Boudin). The probability may be admitted, for the first recognition of a malady as an independent affection does not necessarily imply that the malady is new," &c.

Subsequently this writer observes:

> "In the United States (where the disease may be traced back to the commencement of the century), epidemic cerebro-spinal meningitis became prevalent about the same time that it exhibited great activity in Europe," &c.

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[1] 'Medical Times and Gazette,' April, 4th, 1868.

[2] 'A System of Medicine,' edited by Dr. Reynolds. Lond., 1868.

Dr. B. W. Richardson has suggested diseased grain as a cause of the disorder.[1]

The Massachusetts Medical Society, in its report upon the spotted fever of 1806 to 1815 and the recent epidemic, contrasts the symptoms of the disease, at each of the periods, in parallel columns. The resemblance between the disorder of the early part of the present century and that recently experienced, in both the eastern and western hemispheres, is sufficiently borne out to justify the inference that the diseases were the same.

> "It is in 1805 that, for the first time, we have undoubted evidence of the occurrence of an epidemic of cerebro-spinal meningitis," writes Mr. Collins.[2] "In that year, from February till April, it prevailed in the city of Geneva and its environs, and was closely observed by Vieusseux and Mathey,[3] who in the same year published an account of their experience, declaring that neither they nor their colleagues had ever seen a similar disease. A petechial eruption was a prominent sign, and it is described as a malignant non-contagious fever. An autopsy, in one case, revealed an exudation both upon the upper and under surfaces of the brain, the cerebellum, and medulla oblongata. Of those attacked, the number of which is not mentioned, thirty-three succumbed. This account, in which we find an unmistakable picture of epidemic cerebro-spinal meningitis, possesses a peculiar interest not alone as the first, but as the only available record regarding the prevalence of the malady in Switzerland."

The writer last named mentions in his able report the circumstance that Hirsch himself states that "this disease (*i. e.* spotted fever) had nothing in common with that under consideration," namely, cerebro-spinal meningitis.

> "No more conclusive answer," adds Mr. Collins, "can be given to such a statement than a mention of the fact that, beside the record of the autopsies, the writers on this epidemic, as they gained increased experience, almost universally protested against the designation they had previously adopted, owing to the inconstancy of the eruption which they had at first considered so characteristic of the disease."

Collins traces the several routes pursued by the disease in Europe, North Western Africa, and in America, with much perspicuity; he speaks of it as being non-contagious, or very slightly contagious, and as independent of all geographical limits, local causes, or, indeed, "as yet of any appreciable cause whatever," and of its being "truly a disease, *sui generis.*" Hirsch and other authors dwell upon the tendency of the disease in general to follow the course of rivers, the higher table lands being usually far more exempt from its ravages.

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[1] Before taking leave of Mr. J. Netten Radcliffe's well-written article on "Cerebro-spinal Meningitis," we would notice what is evidently a typographical error at p. 702, line 3. "During the recent outbreak in Philadelphia, it was found that, in the more *as*thenic cases, cupping the nape of the neck was of essential service," &c. The original (Stillé) p. 143, is of course "in the more sthenic cases," &c.

[2] 'Dublin Quarterly Journal of Medical Science,' Aug., 1868.

[3] Cf. 'Journ. Génér. de Méd.,' xi, pp. 163, 243.

In England, where the disease has never as yet, so far as our records tend to show, appeared in an epidemic form—Gervais at Ashburton; Scott at Sunderland; Richardson at Mortlake (Surrey); Whittle at Liverpool; Browne at Rochester; Day at Stafford, have recorded a few cases; also in Lincolnshire, at Devizes and Chelsea it has been seen; and in the metroplis sporadic cases have also been noted by Ogle,[1] Wilks, Murchison, Martin, and Clarke, at considerable intervals of time for the most part, the earliest being Gervis' four cases in 1807, and the last Dr. Wilks' case this year; to these, which Mr. Collins mentions, we would add the four recent cases at Shorncliffe camp mentioned above.

Mr. Ferguson, the Veterinary Surgeon to the Privy Council in Ireland, states that cerebro-spinal meningitis in Ireland has been preceded by and co-existed with an epizoötic of the same nature among the lower animals, especially pigs and dogs. In the disease called purples among the former, so called by reason of the cutaneous eruption which, though at first red, soon becomes purple, there have been found, on *post-mortem* examination, evidences of cerebro-spinal meningitis, besides extravasations of blood into and upon the muscles and viscera, and within the cerebro-spinal cavity.

Splenic apoplexy of oxen and sheep, which in France has been connected with the entrance into the blood from the air of the bacteria, presents somewhat similar phenomena. Dr. Crisp[2] has stated that if the flesh of animals so affected be given to pigs they become the subjects of cerebro-spinal meningitis.

Mr. Collins has illustrated his paper with charts of the temperature, of the pulse, and of respiration, all of which undergo extremely rapid changes in this disease.

In Massachusetts it was observed that the disease showed—

> "Great apparent capriciousness in its predilection for, or avoidance of, military establishments; in its choice of sex; in the varying frequency of the occurrence of spots; in the character of the spots (we may add also by parenthesis, in the groups of its symptoms

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[1] 'Medical Times and Gazette,' 1865, p. 37.

[2] 'Lancet,' July, 1867.

present in different individuals); in fatality, in liability to relapse. It has been generally *consistent with itself* in the matter of contagion; and in prevalence in towns and in the country rather than, or quite as much as, in large cities. It has shown, also, a frequent if not general preference for the earlier period of life."

We do not consider the Massachusetts report by any means rightly represents the treatment of the disease pursued by British physicians, when it states that "in the British practice bleeding and mercury were resorted to."

In Massachusetts, venesection, blisters, and cold applied to the head, have not been approved of; opium in doses of one-twelfth of a grain to young children has been thought of benefit. Permanganate and also hydriodate of potash, quinine, and ergot, have all been tried; quinine in hastening convalescence has had some success.

Dr. Sanford B. Hunt, in an outline of the history of Cerebro-Spinal Meningitis, mentions Dr. S. G. Webber's opinion, that the disease was chronicled in Europe as far back as 1480, and that the jail or hospital fever of Sir John Pringle (1752) was the same malady.

In America it occurred mostly in the winter season, in the ordinary fever haunts of cities, and in isolated country districts, especially those which present the ordinary causative influences of typhus and typhoïd, or are distinctly malarial.

It has also happened on shipboard in association with hardships and confinement between decks. With reference to our present purpose of study—'to learn how far this disease may be connected with military hygiene'—we find it developing itself, especially in the winter of 1863-64, in the army, and also in the succeeding winter. Its most frequent habitat was in overcrowded military hospitals, in cantonments for recruits, and in military prisons. Epidemics occurred especially at Benton Barracks, Missouri, at Newbern, N.C., and at the Penitentiary at Little Rock, Ark.; while at numerous other posts we find records of its occasional prevalence.

In the Confederate army it was witnessed in the army of Northern Virginia, near Fredericksburg, in April, 1863; in Kentucky in the winter of 1861-62; and near Mobile in the closing winter of the war.

The frequent mention of prisons in the above statement will at once attract attention. The observation of our army surgeons, North and South, will confirm the proposition that cerebro-spinal meningitis is not peculiarly a disease of the camp or the campaign.

It sometimes occurred in tents, but most frequently in permanent cantonments, and where masses of troops were aggregated, or in the various prisons. At Little Rock, Ark., the writer saw it in the winter of 1864-65.

Some cases occurred in the camp of a Missouri regiment, which was living in stockaded tents, many of which were damp, ill-ventilated, and, from long occupancy, surcharged with organic matter in process of decay. At the same time, a considerable number of deaths occurred in the Penitentiary, two miles distant from the camp mentioned. This penitentiary was overcrowded with Confederate prisoners. Surrounded by a high stone wall, and itself built of solid masonry, it had very insufficient latrines, and was damp and chilly throughout. All forms of zymotic disease prevailed there, including typhoid fever and smallpox.

The cerebro-spinal meningitis showed itself in the form of a small, compact epidemic, and disappeared after about twelve deaths occurred. All cases were fatal.

At the Benton Barracks, near St. Lonis, Mo., fifty cases were observed by Surgeon Ira Russell during the winter of 1863-64. Dr. Russell says,—

> "At that time the weather was extremely cold; the men were huddled in over-crowded barracks, and subjected to all the moral and physical influences incident to a change from a life of servitude to one of freedom and military service."

In another report, Surgeon Russell details at length the conditions to which these unfortunate men (escaped or emancipated slaves) were subjected. The barracks occupied by them were inferior to the cavalry stables near by, and were most imperfectly heated; while the commissariat and clothing supply was deficient. As in the epidemic at Little Rock, zymotic diseases were very prevalent, coincident and frequently complicated with the meningeal disease.

In the winter of 1862-63 a severe epidemic occurred at Newbern, N.C. It was confined mostly to the 44th, 45th and 51st Massachusetts, and the 10th Connecticut regiments, all of which occupied barracks. The epidemic tendency extended, however, to troops living in tents to a small degree. The regiments most afflicted occupied barracks located on a sandy and sterile plain, elevated perhaps five or six feet above the level of the river. The 44th, which suffered most, was nearest the bank; quite near the bank lay a couple of marshy bogs, small in extent, through which flowed a sluggish stream to the river.

Of the barracks, Dr. Upham says:—

> "They were made of green stuff—hard pine mostly—the logs being taken newly cut from the forest or drawn out from the water, where they had been lying for a few weeks, sawn into joists and

> boards, and used in the fabrication of all parts of the building. . . Thus constructed, they were necessarily cold and damp."

They were also much overcrowded, badly ventilated, and badly heated.

The reader will bear in mind the defects in the hut at Shorncliffe, which we have before mentioned, and the description of the dwellings, by Dr. Sanderson, near the Vistula; and he cannot but be struck with the points of resemblance in these different instances, of the dwellings and localities in both the old world and the new.

In April, 1863, four cases occurred in a single tent of the 22nd North Carolina regiment (Confederate), New Fredericksburg, Va. Three of these cases died, being all from one family of conscripts. The fourth tent mate, an old soldier, recovered. Dr. Robinson says, that it is difficult to define any special circumstances affecting this tent in preference to others.

In February, 1863, eight cases, six being fatal, occurred in the 3rd Alabama (Confederate). No mention is made of the condition of the camp, but we suppose it to have been much exposed: "The season was severely cold." Dr. Hunt then quotes Dr. Sanderson's report in support of what he considers this disease's favoured habitat, namely, cold, damp and overcrowded tenements. Dr. Hunt gives a clearly written account of the symptoms, which he concludes as follows:—

> "Much as it (the disease) varies in the details of its career, it always presents two stages or conditions. The first is a period of wretchedness, in which the tortured organs of sense and consciousness express the keenest forms of pain and restlessness, and of mental hallucination and delirium, rapidly plunging the miserable man into the horrors of convulsion or tetanus, until we find him with eyes fixed or squinting, with the head thrown back, the body in opisthotonos, and the thighs flexed upon the belly, panting, sweating, moaning, or shrieking with pain.
>
> "The second stage is more merciful. The organs of the brain have succumbed under the pressure of effusion, and the exhausted body lies comatose and paralysed. The pain, the muscular contraction, the restlessness, the extreme debility—these are the fixed quantities of the disease. All other phenomena are comparatively non-essential in the diagnosis.
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> "*Mortality.*—Of 713 cases which we have collated with some degree of accuracy, we exclude 347 as being records of deaths only, and not capable of comparison. We have left 366 cases, which present all the data necessary to deduce the average rate of mortality in cases attacked. Most of them are from military life, and include all the recoveries as well as the deaths. The following is the result:

Cases.	Deaths.	Recoveries.	Per cent. of Deaths.
366	243	123	70

It will be seen that this ranks as one of the most uncontrollably fatal diseases, and that the resources of modern science save no more cases than recovered three centuries ago."

"*Post-mortem appearances.*—Of 68 autopsies, the records of which we have examined, all present positive evidence, not only that the meninges of the brain and spinal cord were the localities of inflammatory disease, but that the sufferers died from that inflammation, and not from any intercurrent or other disease which might cloud the discussion of the essential cause of death. The appearances in other organs were mostly negative. As most of the patients had been healthy up to the hour when headache and nape-pain appeared, so we find their organs after death exhibiting trivial signs of disease."

We will quote the leading features in some of the autopsies given in the report. The following is from a report by Surgeon Joseph Jones, Confederate army, of private Goosby's case, of the Georgia militia. The autopsy was performed eight hours after death.

"The dura mater of the brain was normal. The arachnoid was opalescent where it passed over the sulci of the cerebrum and cerebellum. The pia mater was greatly congested, and the larger veins and many of the arteries were greatly distended with dark blood. The pons varolii, medulla oblongata, and spinal cord, were coated with a firm, light greenish-yellow, wax-like, fibrinous exudation; large tracts of the cerebrum and cerebellum were also thinly coated with this effusion, which also surrounded the cauda equina and most of the roots of the spinal nerves, up to their entrance into the dura mater of the spinal cord. The exudation varied from that of a serous fluid to that of a newly-formed membrane; it also varied in thickness, being thick over the pons varolii and medulla oblongata, and gradually becoming thinner as it extended over the convolutions of the cerebrum and cerebellum. The third and lateral ventricles were distended with a light greenish yellow semifluid exudation, resembling pus to the naked eye. The optic thalami and striated bodies, with the walls generally of the ventricles, were coated with a layer of semi-organised plastic lymph.

"Under the microscope, the exudation presented different stages of organisation and development, from the simple granule to the perfected fibre, and even fibrous tissue."

Dr. Jones comments upon the obstacle such a structure became in the nutritive membranes of organs essential to life, surrounded by a bony case. Goosby's illness had lasted two weeks, the intensity of the inflammation had ceased, and much of the effused material was becoming organized. The liver contained grape sugar, which is absent, Dr. Jones observes, in the liver of malarial fever. The organs of the thorax and abdomen may be said to have been healthy, or so nearly so as to present no evidence that they shared in producing the fatal result.

There was no complication with malarious disease. The products of the inflammation resemble in all respects those of pleuritis and peritonitis. The inflammation was confined chiefly to the pia mater, involving, to a certain extent, also, the arachnoïd.

"The nervous elements of the brain, spinal cord, and cervical nerves (ganglionic cells, commissures, nerve-tubes, and nervous connective tissue), appeared to be free from inflammatory action."

Dr. Jones considers that primary inflammation of the nutritive membrane of the brain and spinal cord explains the rapidly fatal effects of the disease upon life, and that if the brain and cord were the first attacked the effects would be less rapid and fatal. He thinks, too, that the results of inflammation explain the sequelæ of the disorder.

Exposure, cold, damp, and the hardships of camp-life, he regards as causing the disease, just as they would produce pleurisy, peritonitis, pneumonia, or acute rheumatism.

He says it differs from a contagious malignant fever, and regards it, not as a pyrexia, but as a phlegmasia. Our readers may remember that we quoted the opinions of French authors, who held like views as to the nature of the disease, in the October[1] number of the Review, but we also showed Dr. Kleb's opinion as opposed to this idea, and his reasons for dissent therefrom.

Dr. Webber, in his Boylston Essay of 1866, divides cerebro-spinal meningitis into three orders: in the first he states the cerebro-spinal system is affected; in the other two it is not. As Dr. Hunt observes, this is a classification on the *lucus a non lucendo* principle. In the second or pneumonic form, there is less disturbance of the cerebral functions; *delirium is wanting*, though headache may exist. With great debility are found the symptoms, rational or physical, of pneumonia. Petechiæ occur.

Opisthotonos is not usually met with—"Only two cases of this form occurred among those recorded; both recovered; both exhibited petechiæ."

The third variety Dr. Webber pictures as marked by the absence of all the peculiar local symptoms; having the debility, the petechiæ and the complications. Two cases occurred: one with suppuration of both parotids, the other with great swelling, and excessive tenderness of the right knee; *both were without cerebral symptoms;* both recovered.

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[1] 1868.

Dr. Hunt again observes, these cases did not appear to have been cerebro-spinal meningitis, and that, these cases excepted, the two hundred and forty-nine cases included in the tables of Dr. Webber seem to have been genuine. The four mistakes do not affect his records of autopsies.

Of these he says: "In the lungs, the only lesion which is at all general, is a congested state," which he ascribes to hypostasis. "Sometimes there is a thick deposit of lymph upon the pericardium, and sometimes the same substance is to be found in one of the cavities,"—a fact important as a possible metastasis.

Surgeon Ira Russell, at Benton Barracks, St. Louis, Mo., records seven *post-mortem* examinations. In one of these he mentions splenic enlargement and fatty liver; in a second, softened mucous membrane of the stomach and congestion of both lungs; in a third, slight pleuritis and slight inflammation of the left lung; in a fourth, lymph covered the left pleura, and the left lung was in grey hepatization, with the liver engorged; and in a fifth case, the whole intestinal mucous membrane was softened.

"In two cases lumbrici were found,—a reminder of the French records of early epidemics, when this parasite was considered a cause of the disease. The cases coming under Dr. Russell's observations were peculiarly malarial; yet we find in two of his cases that no complication was present. Those complications that were exhibited are phlegmasiæ, or else chronic disease, and none of them indicate any of the known forms of febrile disease.

Surgeon Upham observed in one case, apoplectic points in the lung, the spleen double the natural size, and Peyer's patches more prominent than usual, with ulcerative points in one gland; "not having, however, the legitimate appearance recognized in typhoid fever." In a second case the lungs were tubercular, with apoplectic spots, the liver enlarged, and the spleen twice the usual size. In a third case, which lived nearly a month, Peyer's patches were very much thickened, and in one or two instances, ulcerated. In a fourth case, there is no evidence of cerebro-spinal meningitis. The patient died suddenly, comatose. The brain was found softened, and the liver in fatty degeneration.

The American reviewer considers these lesions of the organs of the chest and abdomen as accidents, and not essentials; and he goes on to state that inflammation of the meninges, especially of the pia mater, is the characteristic of the disease. He quotes several authors to show the uncertainty of the occurrence of petechiæ in disease, and observes, that the flushed and livid skin, the hemorrhagic spots beneath its surface, and in the conjunctivæ or lungs, stomach, or peritoneum—

> "Constitute a hæmorrhagic effusion, dependent upon disorganised blood, which itself is dependent upon a specific poison is, thus far, not proven, nor does the evidence yet adduced assign to these occasional blood-spots any higher importance than may be conceded to the much more common herpetic eruptions about the lips."

The outline of treatment given from the records kept by the several medical officers quoted in memoirs does not differ to any extent from what we have already noted, especially when noticing the American and French practice in the preceding part of this review.

> "In all peculiarly fatal diseases, the modes of treatment recommended are numerous and contradictory. Not only do they vary with the theories that practitioners may entertain of the pathology of the disease, but with that feeling of irresolution or desperation which prompts strange remedies in a strange disease. Accordingly, we find an almost endless, certainly an absurd, catalogue of remedies recommended by writers," &c.

The "epidemic constitution" during the existence of cerebrospinal meningitis, is felt by many in the vicinity, "who do not finally develope the full phenomena of the disease. Headaches, neuralgias, slight stiffness of the muscles, and soreness of the cutaneous surfaces, are common among those who are classed as well, but are subjected to the same atmospheric influence," &c.

The disease, as above mentioned, is regarded by Hunt as a phlegmasia, grave in consequence of the importance of the part implicated; he does not consider the disease contagious.

The disease, "then," continues the Massachusetts report, "is clearly not exclusively, or *par excellence*, a *military* disease." To what proportionate extent military establishments, and particularly recruits, are liable to it remains to be ascertained.

That the disease is *not* one of large cities, comparatively, is the general result thus far.

In France it has been stated that neither *climate* nor *season* has been found to exert any influence upon the disease. In Leipzig, an epidemic occurred in the summer. But in Ireland and America, by far the greater number of cases hitherto reported have been in the first five months of the year.

In favour of its non-contagious nature, the single cases, respectively in a boarding-school at Brattleboro', Vt., and in the Mississippi squadron, are cited.

Negroes have suffered from this very fatal disease.

Dr. Page makes an interesting statement as to the behaviour of the disease at Gallop's Island. He says, the September case occurred at a period of great over-crowding. This over-crowding was abated, and nothing more was seen of the disease till January 29th, when the barracks were again over-crowded, there being from 130 to 180 men in a barrack intended for 100. Dr. Page presaged the renewal of trouble, and reported the danger to the Government, begging that the over-crowding might be relieved, but in vain. The cases went on appearing one after another, till the surplus number of men were shipped off, when it ceased. Dr. Page expresses a decided opinion that the disease at Gallop's Island arose from over-crowding.

On the other hand the case of the City Hospital is cited, where there was no crowding, and the ventilation was stated to have been admirable. The mass of evidence afforded in America on the influence of locality, of damp or dryness, and of contagion is contradictory.

As to duration, one case is stated to have lasted but "thirty *minutes* from the acute attack." The patient had, however, vomited a few hours previously, from, as it was supposed, an over-loaded stomach; another case is given of twelve months' duration; this patient, it is said, "died in one year, after months of activity, but not of recovery," Relapses, many being fatal, frequently occurred.

Cold, combined with moisture, appears, by an examination of the 287 cases, to be that condition most favouring the production of the disease.

Evidence that the disease occurs by contagion or in damp localities is by no means afforded in the Massachusetts reports.

Convulsive action, in one form or another, was present in considerably less than half the cases in which the symptom was mentioned.

Headache, in some instances of an excruciating nature, was present in a vast majority of the cases in which the symptom was mentioned, or its presence or absence could be ascertained.

Delirium was present in 165 of the 287 cases recorded; it was described as mild, violent, low muttering, &c.; and in the large majority of the cases of delirium the patients could be roused to give logical answers.

Opisthotonos was absent in but 48 of the 287 cases.

Pain or tenderness of the neck was recorded in the great majority of cases.

The respiration was affected (in many instances only functionally) in a great number of cases.

The pulse was occasionally retarded, sometimes variable, often accelerated. In some instances it was not much affected.

Nausea and vomiting are not unfrequently spoken of.

Morbid appearances of the skin were not mentioned in 29 cases, not looked for in 7 and not recorded in 2 cases.

They were present in 129 cases, very general in 19, not present in 84, not marked in 6, very general in 19, and in 11 there was a peculiar condition of the skin, in some cases without spots.

Petechiæ and *opisthotonos* were often combined in the same individual.

The termination was not stated in 9 cases, was fatal in 170, and recovery took place in 108 instances.

The autopsies, in well-marked instances of the disease "tell the same story of meningeal inflammation."

The treatment varied, being "antiphlogistic, stimulant, and supporting, that of liberal drug medication; perturbating; expectant. But we are unable to generalize in favour of any of these. That which has been followed by a happy termination in one group of patients, has in another set been less fortunate."

The following short notes having reference to affections of the cerebro-spinal meninges, are apparently worthy of mention in connection with the recent epidemic. Some of Dr. Trayer's cases corroborate the observations of many in reference to the insanitary character of localities in which the disease occurred.

It is well, also, to bear in mind the fact, that sporadic cases have from time to time occurred, in the opinion of many, in different parts of the world; and this opinion is corroborated by the writings of Cruveilhier and Abercromby. Cruveilhier[1] saw many cases of what he designated "Meningite sous-arachnoïdienne comateuse"; he considered that when the meninges over the convexity of the hemispheres is inflamed, it is, according to his experience, characterised by stupor gradually increasing to coma; whilst headache, with increase of sensibility, delirium, and convulsions, are strangers to it.

Dr. Abercrombie[2] notices a modification of this disease,[3] remarkable from its rapidity and from simulating hysteria, or even mania; although on examination it presents merely a highly vascular and injected state of the pia mater, complicated in some instances with slight subserous effusion. It commences with depression of spirits, to which cheerfulness soon succeeds, but finally passes on to delirium. The patient's manner is hurried, he talks rapidly, wandering from one subject to another. Obstinate watchfulness exists throughout, the pulse is small and rapid, the countenance pale and expressive of exhaustion. The treatment is founded on these circumstances.

Stimulants cautiously given afford relief, while depletion aggravates the evil, or produces but a temporary alleviation.

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[1] 'Cyclopædia of Practical Medicine,' vol. i, p. 286.

[2] Ibid., p. 287. [3] Acute meningitis.

Seven cases of "the so-called cerebro-spinal typhus," are recorded by J. J. Trayer, M.B.,[1] as treated by him, in the Bagnalstown Hospital; in five of the cases, by five- or ten-grain doses of bromide of potassium. Four of the latter recovered. Three of the cases occurred in one family inhabiting a house of "bad sanitary character." The first of these last-named cases, was that of a lad of twelve years, who had walked fourteen Irish miles, and getting his clothes soaked with wet, had allowed them to dry on him.

Concluding General Remarks.—Epidemic Cerebro-spinal Meningitis, as the disease treated of by the several authors noted above, is now most generally termed, is supposed by some to have occurred in past ages as well as in more recent times.

Authors are by no means agreed as to its exact nature. Many regard it as a form of fever. Some designate it as sinking typhus, and others think it a complication of fever. Many view it as connected with malarial fever. That it greatly differs from typhus, typhoïd, or ordinary malarial fever none, we believe, will deny. It was thought by many observers during its recent partial prevalence in Ireland to hold some sort of relation to cholera. The sudden and overwhelming nature of its onset, and its often rapidly fatal ending, the shivering and vomiting, abdominal pain and sudden prostration; its high rate of mortality, and the fact that it appeared in Ireland just before the last outbreak of epidemic cholera, and upon the cessation of that disease, in both cases in the colder months of the year; these circumstances tend towards the idea that some sort of relationship exists between the two diseases.

Opinion is quite divided as to whether the disease is local or general in its nature.

Post-mortem examination discloses morbid changes in the contents of the cerebro-spinal cavities in the great majority of cases. The detection of pus may often require an expert microscopist. Mayne's observation, that the arachnoïd is in general exempt from pus, is corroborated by Klebs; the pia mater, the latter observes, is the exclusive seat of purulent meningitis. Cases of destruction of portions of the spinal cord have, however, been met with by several observers.

Rather conflicting evidence is given by different writers as to the temperature of the body, and the prevalence of cutaneous affections in epidemic meningitis. In these respects, as well as upon other points, we may suppose that epidemics varied in different districts, just as individual cases, even in the same locality, differed much in their course and symptoms.

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[1] 'Med. Times and Gazette,' Nov. 9, 1867.

As to treatment, it is well to bear in mind that the disease does not properly come within the class of the phlegmasiæ, and that experience has not warranted the adoption of depleting measures, at least, of at all a general nature.[1]

Encouragement has been given for the use of bromide of potassium; opium has also been recommended. A supporting plan of diet rather than stimulation, appears the most advisable. Cold to the head and spine have been well spoken of by some, and found of no use and even injurious by others. In some cases presenting symptoms of cerebro-spinal meningitis *stimulating applications to the head* have been used with benefit.

The questions of causation and contagion may be said to be veiled in obscurity, the evidence upon both subjects being so contradictory.

Under the heading of Suppurative Choroiditis,—Panophthalmitis, Von Carion observes:[2]

> "There is, however, no doubt, that true suppurative choroiditis may be also secondarily developed by transplantation of the inflammatory process from the conjunctiva, cornea, iris, &c., upon the choroid. In the epidemic cerebro-spinal meningitis, and in many cases of typhus, puerperal fever, pyæmia, accompanied by meningitis, the inflammation appears to go from the meninges, along the sheaths of the nerves, to the internal parts of the globe, and here to give rise to abundant inflammatory products. It is true that the ophthalmia appearing under the last-named conditions are not always exactly the same in character. In certain cases, we have only a severe catarrhal conjunctivitis, which, subsequently, may be combined with ulcerations of the cornea, and thus possibly destroy the eye. Similar agencies, perhaps, may work together here, as in the neuro-paralytic ophthalmia, since the dangerous condition is first seen when the lids are opened, and the nervous conduction has become limited, and very many injurious influences act upon the exposed globe (Schirmer); in other cases the ophthalmia has from the beginning the character of a suppurative irido-choroiditis, and is developed with severe inflammatory symptoms, with intense redness, swelling of the lids and conjunctiva, which always quickly leads to the adhesion of the papillary margin to the anterior capsule, and causes large purulent

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[1] We object to the statement of "a correspondent" in the 'Medical Times and Gazette,' May 25th, 1867, p. 561, that "In the French epidemic large bleedings, in the early stage of the disease, were the only means of treatment attended with success." Such treatment has not been approved of in America or in Ireland, and in Germany; Klebs has pointed out his reasons for dissenting from it, citing the experience of French physicians against it, as will be seen by reference to this 'Review,' p. 386, Oct., 1868.

[2] 'Treatise on the Diseases of the Eye," by Carl Stellwag Von Carion, M.D., Professor of Ophthalmology, Vienna; translated by Drs. Hackley and Roosa, New York: London, Hardwicke, 1868. Pp. 255-6.

products, which are partly exuded into the anterior chamber as a hypopyon (Kreitmair), and partly infiltrate the vitreous humour, especially its anterior portion, while the greater impairment or entire loss of vision announces the internal participation of the retina, choroid, and optic nerve. The infiltration of the vitreous is betrayed by a very marked bright yellow reflex, which evidently proceeds from a thick opacity, lying immediately upon the posterior surface of the lens, and which, from its want of vessels may be plainly distinguished from a detached portion of retina lying upon the crystalline body (Jacobi). The reflex is recognised as characteristic, since it is never wanting unless the lens has been already involved in the process of proliferation, and has become cataractous, or unless inflammation is not yet completely developed, but remains confined to the iris, without affecting the deeper parts of the eye. Such suppurative inflammations of the eye have been seen as a result of meningitis in the various stages of the disease (Jacobi), the occurrence of which is explained by the conduction of the proliferation process along the sheaths of the nerves. Pathological investigations support such a view (Schirmer). Some epidemics of these acute exanthemata[1] are peculiarly marked by the frequency of such suppurative inflammations and other purulent depositions of the eye.

"Now and then an ophthalmia, appearing in consequence of epidemic cerebro-spinal meningitis, has the character of a metastatic process. Some, on the basis of epidemics observed singly, hold this to be the rule, and support their views by the fact that here, as in pyæmia, purulent serous exudations in the joints, pneumonia, pleuritis, &c., are frequent complications (Jacobi).

"In the variety of suppurative choroiditis seen in epidemic cerebro-spinal meningitis, atrophy is the ordinary result, and is often evident at an early period by a very marked softness of the globe. Still, suppurative perforation, and, as a further consequence, phthisis of the globe has been observed (Lindström)."

The phthisical destruction of the globe is not always carried on in the same way. Generally the cornea is infiltrated, changed into a mass of pus and deliquesces. Then the contents of the globe are to a great extent excavated, and the remains suppurate away.

The tension of the distended eyeball often causes the cornea to burst with an audible snap, and the contents of the globe are spirted out to some distance. The cornea, with or without a part of the sclerotica, undergoes necrosis, and thus the contents of the eyeball escape. Sometimes this occurs through an enlarged vessel of the sclerotic, or this latter membrane has to a greater or less extent actually separated. On the bursting of the eye the intense pain which had accompanied the distension of the ball generally, but not invariably, ceases. At last the globe shrinks more and more together, inflammation recedes, suppuration ceases, and the perforation closes. The atrophied stump dwindles to the size of a pea or hazel-nut, with a smooth or wrinkled appearance; and generally appears deeply sunk in the orbit. The lids are retracted, immovable, and closed. In young persons actual diminution of the orbit may even cause marked deformity.

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[1] Anomalous variola, Scarlet fevers, Typhus, &c.

The chief therapeutic indications are: to limit and suppress the rapidly advancing process of proliferation, and with this to remove everything which may excite and maintain the suppuration; furthermore, to subdue the inflammation by direct means, and, as far as possible, to favour the resolution of the morbid processes already existing.

PART SECOND.

Bibliographical Record.

Art. I.—*Transactions of the Clinical Society of London.* Vol. I. London. 1868. Pp. 197.

The institution of a new society in London for the promotion of medical and surgical science may, to some on-lookers, appear uncalled for, and on a par with the establishment of new hospitals, such as crop up yearly to cure a few selected forms of human ills which the originators assure the public are the most sad and the most prevalent in the whole list of diseases and, as they inferentially insinuate, such as pre-existing hospitals and dispensaries have cruelly overlooked, neglected, and despised.

The Clinical Society of London arose suddenly into existence, and the fact that it, from the first, attracted to itself a very large number of the best known physicians and surgeons of the metropolis, must be accepted as an indication that such an association was needed, that it proposed to accomplish some end, to carry on work of a particular sort, or in a manner different from that contemplated in the operations of any other society previously constituted. It may be said to be the representative society of young physic! The names of the provisional committee entrusted with its formation are those of the young, or comparatively young, physicians and surgeons of the metropolitan hospitals, known for their active prosecution of medical research, and they wisely selected for their apologist and inaugurator the deservedly popular and illustrious Sir Thomas Watson, Bart.

After a brief notice of the first dawn of the society and a list of the members, this, the first volume of transactions, appropriately commences with a report of the address of the President, devoted to the enunciation of the objects of the newly founded association. In this address Sir Thomas Watson remarked that the society seemed to him "well calculated, gradually to bring about that which, in his judgment, is the thing most needful at present among us; to wit, more exactness of knowledge, and therefore more direct and intelligent purpose, and more successful aim, in what is really the end and object of all our labours—the application of remedies for the cure or relief of disease."

A claim to promoting exactness of knowledge would be put in by all the other medical societies established, as a prime purpose of their existence: consequently the President said no more of this very general object of scientific bodies, but proceeded throughout the remainder of his discourse to expand the latter portion of the paragraph quoted, insisting most justly upon the uncertainty of therapeutical knowledge and therapeutical aim, and moreover pointing out certain special problems in therapeutics especially demanding solution, and calling for the best efforts of the members of the society for elucidation. He concluded by an encouraging prognosis, remarking as follows—

> "I think I do not entertain an extravagant expectation of the results of the formation of this Society, when I express my belief that, if wisely and strictly managed, it will hereafter be spoken of as the starting-point of a vast and solid improvement in that which is our special office in the world, the scientific and intelligent exercise of the divine art of healing."

This first volume of Transactions may be referred to in determining how far the prognosis has hitherto proved correct; how far the society has kept true to the principles of action enunciated by its distinguished president. And we are obliged to pronounce the opinion that the therapeutical line of observation and research has been very partially pursued. A most excellent and instructive collection of cases have been put on record, and thus far, at least, it has fulfilled the object primarily conveyed in its name, the "Clinical Society;" but similar repertories of cases may be found in the transactions of the old medical societies, in the volumes of hospital reports, and in the various journals. The therapeutical aim and purpose of the society do not stand prominently out, but rather special pathology. It may be that Sir Thomas Watson was the exponent rather of his own views of the special scope of the society, than of those of the founders. In that case our criticism does not apply, and we must conclude that the writers of the papers now published, most of whom were concerned in the formation of the society, have conceived that their prime object has been increased exactness in recording cases and the collection of interesting and unusual instances of disease,—a highly commendable purpose, but nevertheless one that falls within the compass of previously instituted medical societies.

Far be it from us to say anything depreciatory of the communications contained in this volume. On the contrary we commend them to the hearty acceptance and careful consideration of our professional brethren. The volume reminds us of that issued early in this century by an association of physicians and surgeons for medical observation and research, which continues a valuable monument of their assiduity and zeal in their profession. In like manner the present and the many subsequent volumes we trust to see issued by the Clinical Society will command the gratitude and admiration of future generations of practitioners.

There are in all thirty-three communications, medical and surgical and therapeutical. Excluding surgical operations, which in one sense, as measures of relief, might be accounted therapeutical, recorded cases illustrating particular modes of treatment, or the utility of certain medicines are few. Of such may be mentioned Dr. Marcet's case of temporary loss of voice, successfully treated by galvanism; Mr. Campbell de Morgan's paper "On the Use of the Chloride of Zinc Solution in the treatment of abscess connected with diseased joints, and Dr. Handfield Jones' notes, in recounting the history of a case of cardiac hypertrophy, on a remarkable effect of digitalis.

Most of the communications are commendably brief; the longer ones are:—Dr. H. Weber's, "Two Cases of Sudden Death from the Nerve-centres in Rheumatic Fever, with excessive temperature before death," followed by notes of two other such cases by Drs. Murchison and Sanderson; Dr. H. Greenhow's "Cases of Intermittent or Paroxysmal Hæmaturia;" and Dr. Fagge's "Cases of Parasitic Diseases of the Nails, associated with different parasitic affections of the scalp and body."

Two considerable and valuable papers on "Motor Asynergy," appear, by Dr. Headlam Greenhow, but the cases given in illustration have heretofore been published by Mr. Lockhart Clarke, in an essay on locomotor ataxy contributed to the St. George's Hospital Reports, Vol. I.

The character of the communications being for the most part descriptive, it would be out of place to analyse them in this review; many of them would indeed not bear abridgment, and of all it may be affirmed that they will be best understood when read as they stand in the words of their several authors. Where the case detailed suggests it, the pathology is often ably discussed, and the comments or remarks with which the writers conclude their communications are of much value and interest.

But there is a feature in the working of this society which deserves notice, nay, more, calls for imitation on the part of other learned societies,—viz., that of referring cases presented for consideration to a committee, or, in other words, to specially chosen reporters, after the manner so long in use in French scientific societies. It is worth while here to state the scheme, as proposed by the Clinical Society. Communications made are of two classes, the one, "relating to cases of which the records are complete;" the other, "to cases still under observation." It is in the power of a member to ask for a committee to be appointed, with the sanction of the president, to co-operate with him in investigating cases of the latter class. This committee reports on the result of its investigation, and the report is read after the narration of the case to which it applies by the member communicating it. The president, likewise, may nominate from time to time committees of at least three members, for the purpose of conjoint investigation of clinical, and especially of therapeutical questions, their reports to be based on clinical observations and to "consist of three parts, viz., 1. Cases under observation; 2. Summary of facts observed; 3. Conclusions."

Much completeness and accuracy in the detail of cases are required by Rule xliii, and by Rule xlv it is ordered that—

> "Every communication shall conclude with explanatory remarks by the author; and, whenever it is possible, the facts recorded should be demonstrated by the exhibition, either of the patient, or of photographs, drawings, or casts, and also by microscopical and chemical analyses."

The authors of the now published communications have sufficiently observed these rules and recommendations, and if the same course be faithfully followed in future years, we shall certainly be in possession of an admirable collection of cases, from the analysis and collation of which important doctrines in pathology and therapeutics may be arrived at. In the present volume there is an absence of illustrative drawings, except, indeed, if they count as such, a few sphygmographic diagrams. But the transactions of the society will, we trust, in future years be illustrated; for pictorial description often conveys to the mind a much clearer conception of objective facts than can any amount of verbal description. We are called upon to remember that the Clinical is a very young society, and that the publication of its transactions unadorned is a sufficient venture. We readily recognise the force of this apology, but at the same time trust it will find itself strong enough and rich enough, on the next occasion it issues a volume of transactions, to introduce figures in illustration of those communications that require or are better for it. Lastly, we hope further that the society will have, in ensuing volumes, some reports by committees on therapeutical questions, and therein fulfil that principal end and aim of its foundation, the advance of therapeutical knowledge, so forcibly advocated by its distinguished president.

ART. II.—*De l'emploi du Spéculum Laryngien dans le traitement de l'Asphyxie par Submersion, &c.* Par A. DE LABORDETTE. Paris, 1868. Pp. 23.

On the Employment of the Laryngeal Speculum in the Treatment of Asphyxia by Submersion, &c. By A. DE LABORDETTE.

THIS *brochure* is a reprint of an article in the 'Annales d'Hygiène

Publique,' and has for its purpose the advocacy of the employment of a two-bladed dilating speculum in endeavours to resuscitate the drowned. The instrument was devised by M. de Labordette for effecting examinations of the throat; and he asserts that it is easy of introduction, and seldom induces efforts to vomit; that it enables the user to examine with facility the epiglottis, the superior aperture of the larynx, and the pharynx, either directly or with the aid of a mirror affixed to it; and that, moreover, it facilitates the introduction of instruments to those parts for the purpose either of removing mucosities or false membranes, or other matters, or of effecting catheterism of the trachea. But besides all these excellent objects that it is calculated to fulfil, he insists, in this essay, on its value in cases of asphyxia. A commission of the Academy of Medicine reported favorably of the instrument; and on the subsequent recommendation of M. Auguste Voisin, "Directeur des Secours Publics," the speculum has been added to the "boîtes de secours," the collection of approved appliances kept in towns, by authority, for employment in all emergencies affecting human life.

This history is sufficient to recommend the instrument to due examination and proper trial.

Before detailing the plan of using it, M. de Labordette enters on a discussion of the phenomena of drowning, and comes to the conclusion that death does not result, at least in an important degree, from water drawn into the air-passages, but rather from the exclusion of air—the suspension of respiration—in consequence of a sort of spasmodic action of the organs concerned in inspiration, of the closure of the mouth and the position assumed by the tongue and palate, all together concurring to prevent the ingress of water into the larynx. The spasmodic closure of the mouth he regards as a good sign, because that so long as it exists there is good hope of recovering the patient; whilst when it is relaxed and falls open, death may be assumed complete. This rigid closing of the mouth, with its accompanying rigidity of the limbs, is a totally different phenomenon to that of the rigor mortis which follows at a later period, and after a previous stage of muscular relaxation. Hence he contends for the necessity, in all cases of drowning, of opening the clenched mouth by means of a wooden lever or wedge, and of introducing his laryngeal speculum, both with the view of cleansing the pharynx from the collection of mucus encumbering it, and also more especially for the purpose of allowing the free admission of air into the lungs.

This proceeding is supplementary to other measures commonly pursued, and is not intended to replace them; at the same time the neglect of it M. de Labordette holds to be one reason of the failure of the Marshall Hall and Sylvester plans for restoring animation. The necessity of first overcoming the spasmodic closure of the mouth before the attempts to re-establish respiration are made, is overlooked. A further recommendation the author advances is to make and remove pressure alternately on the epigastrium, as a means of re-establishing respiratory action. Two wood engravings are introduced, to show the construction and appearance of the instrument, both when open and when closed.

The preceding notice will suffice to call attention both to the ingenious instrument described, and to the excellent review of the phenomena of drowning contained in the *brochure.*

Art. III.—*Guinea-Worm or Dracunculus—its Symptoms and Progress—Causes—Pathological Anatomy—Results and Radical Cure.* By James Africanus B. Horton, M.D. Edin., &c. London, 1868. Pp. 51.

Now and then a case of Guinea-worm occurs in the practice of medical men in this country, in the person of some sailor or traveller from the West Coast of Africa, or other tropical part of the world where the worm is common. It is, therefore, desirable to gain an acquaintance with the medical history of the entozoon, with the consequences resulting from its presence in the areolar tissue, and, above all, with its treatment. To those seeking this knowledge this pamphlet will be of service. Its author speaks from personal experience as a practitioner on the West Coast of Africa, and is, consequently, a safe guide on practical details. In scientific details respecting the development, the probable transformations it undergoes, and the minute structure, he cannot, indeed, be accepted as an authority. For instance, the following is his anatomical account of the animal:

> "The full-grown worm looks like a white cord, small at both ends and large towards the middle, especially the *middle third from the tail* (?). The worm may be divided into two parts—a head and a tail. The head terminates abruptly in a cruciform mouth, by which it extracts food from the surrounding tissue; the tail tapers, and terminates in a hook-like process of very delicate texture. The body of the worm is divided into two chambers, viz. the alimentary canal, which is placed in the centre, and by far the smaller chamber; and the larger one, situated between the alimentary canal and the outer coat of the worm, which is filled with a white milky fluid, holding in suspension myriads of microscopic embryo (*sic*)."—Pp. 13, 14.

Probably from distrust of his own descriptive anatomical powers he adds an account of a dissection of a worm made by Dr. Bastian, which fortunately does convey a more clear conception of the matter.

The worm may lie dormant for a few weeks only, or for several months, or, indeed, for a few years, according, it would seem, to the sufficiency and suitableness of the pabulum it meets with. At length it seeks to effect its escape, having now grown to the length of from two to four feet, and being distended with its young; and in attaining its purpose sets up inflammation at some point under the skin, which leads to the formation of a vesicle or pustule, which, when opened, gives issue to a small purulent or ichorous discharge, and reveals the worm to view. The object then is to lay hold of the worm, to fix it to a piece of a quill or rounded stick, and by daily traction and winding to effect its removal.

The influences actuating the worm to leave its feeding-ground in the areolar tissue are thus quaintly conceived and stated by Dr. Horton:

> "The attempt of the worm to relieve itself from the tissues of the body is instinctive, the general cavity being loaded with myriads of embryo (*sic*), of matured size, produces excitability and *a feeling of great weight* in the worm, which causes it to endeavour to get out of the system."—P. 16.

A curious feature in the history of the animal is, that it at times migrates from one part of the subcutaneous tissue to another; yet it seldom leaves that tissue. Thus in 300 cases recorded it is stated to have infixed itself in the tongue in only one case, and in the testicle (or rather, as we believe it should have been stated, in the scrotum) in nineteen cases. The interior of the eye has also been stated as an occasional locality: but we think Dr. Horton is correct in rejecting the supposed cases of this sort, and in regarding the animal so found to be the filaria of the eye.

Six instances of Guinea-worm are detailed from observations made by the author himself. In the last described he remarks that the

> "Hypertrophy of the scrotum is so firm and massive, that it formed the germ of that dreadful disease called *elephantiasis scroti*. Without knowing the history of the case on first seeing the testicle, one will at once place the disease on that head: in fact it had now changed its type, and elephantiasis scroti is being formed. The Guinea-worm, therefore, is excitant: and this is not the only case I have seen where an attack of Guinea-worm was followed by such elephantiasis."—P. 26.

Yet at a subsequent page (p. 45) he refers to the hypertrophy of the scrotum excited by the worm as something different from true elephantiasis of that part. Another dictum, which we are inclined to doubt, is that every one who has elephantiasis has previously had Guinea-worm in the part affected.

It is satisfactory to learn that there is a sovereign remedy for Guinea-worm, which acts as a poison to the parasite, and is at the same time a safe medicine. This is assafœtida, given in the form of tincture, in doses of thirty drops three times a day. If experience confirms the efficacy of this simple treatment, Dr. Horton will, indeed, well deserve the thanks of the profession and of the public, for having published his pamphlet, defective though this be in lucidity.

ART. IV.—*Annual Report of the Commissioners for Administering the Laws for Relief of the Poor in Ireland.* Including the Twenty-First Report under the 10 & 11 Vic. c. 90. And the Sixteenth Report, under the 14 & 15 Vic. c. 68, with Appendices. Dublin: A. Thom, for Her Majesty's Stationery Office, 1868.

It is pleasing in these days to be able to turn to any record of facts connected with the real state of the Irish nation.

Though a slight increase in the number of workhouse inmates occurred in the winter of 1867-8, a decrease in the number of sick and in the mortality, as well as a small decrease in the number of able-bodied inmates in workhouses took place.

The open character of last winter, and the unremitting industry of the agricultural population, seem to promise a continuous decrease in the numbers in workhouses.

> "Upon the whole it should be regarded as satisfactory that the political disturbances of the last two years have been attended with so little apparent increase of distress, and by so little abatement, if there be any, of confidence in farming euterprise.
>
> "The farm rents, according to all information on the subject, have been most promptly and cheerfully paid during those two years, and the poor rates certainly were never more easily collected."

The population in the manufacturing districts were not so favorably circumstanced last year as the more agricultural portion of the people. Speaking of distress occurring in some parts of Ireland—owing to the rise in the price of bread-stuffs—and the appeals for relief made frequently through the public press, the report states that—

> "It is to be apprehended that this form of distress occurring locally from time to time in certain districts of Ireland, and being made the subject of very exaggerated descriptions in the public press, has had the effect of misleading many persons as to the actual present condition of the great bulk of the population of Ireland, who live by agriculture, and in causing to be overlooked the very

rapid progress that population has made during the last eighteen years in improved comforts, as regards their food, their clothing, and their dwellings."

Again—

"The researches of Dr. Edward Smith, and other scientific authorities on dietetics, have established the fact that the Irish farm labourer is better fed, and at far less cost, than his representative in England."

"The peasantry of Ireland are now far better clad than they were twenty-five years ago. Their dwellings, too, have undergone a great improvement; more than 400,000 of the worst class of cabins are known to have disappeared from the face of the country since 1841. It is true that the class next above the lowest is still the dwelling most in use. This class, though ill-lighted and much inferior in cleanliness and comfort to the English cottage, has one sanitary condition which atones for much of its apparent discomfort. The one room which serves for both day-room and dormitory, occupies the whole length, breadth, and height of the building, and is ventilated all day by a draught from the open door to a blazing turf fire under an ample chimney.[1] At night, even when the door is shut, there are crevices which, with the aid of the never extinguished fire, effect a continual process of ventilation. Above all, it is not ceiled, but open to the thatched roof, through which the foul air which has been breathed is subtle enough to escape outwards. Compared with the ceiled bed-rooms of the indigent poor resident in towns, the thatched cabin of the Irish is comparatively free from that most active and fruitful source of fever, the presence of a highly vitiated air within the dwelling. Accordingly, the rate of mortality by fever in unions containing large towns is more than twice that of unions with a rural population; and nowhere is it greater than in Dublin."—p. 15.

Very satisfactory progress has been made in improving the sanitary state of Ireland, as regards epidemics; but much still remains to be done, especially in reference to fever, scarlatina, and measles. Suitable hospitals exist for the reception of all such cases, and by this means, when adopted in time, both for the benefit of the patients themselves, and for that of their families and neighbours, much has been achieved; however, much difficulty often occurs in inducing the working-classes to send their children to hospitals when attacked first with these diseases.

The working of compulsory vaccination, carried out by the system of dispensary medical relief, has been attended with highly satisfactory results. In the year ending 30th September, 1864, the number of cases of smallpox attended by medical officers of dispensary districts, was 1965, with a mortality of 854; in 1865 the numbers were respectively 2090 and 347; in 1866, 579 and 187; and lastly, in 1867, 105 and 20. Upon this important subject we will quote the Commissioners' report:

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[1] See also C. F. Moore, M.D., in 'Dublin Quarterly Journal of Medical Science,' 1854, "Cases in Practice."

> "When the Irish Compulsory Vaccination Act was passing through Parliament, the remark was made that similar provisions had been in force in England for ten years, under Lord Littleton's Act of 1853, and had not been successful. It appears right at this time to point out how much the success obtained in Ireland has been due to the machinery adopted for carrying out the compulsory provisions of the Act.
>
> "The dispensary medical officers to whom the duty of public vaccination continued to be confided exclusively, became at the same time registrars of births, under the Births and Deaths Registration Act,—this Act and the compulsory Vaccination Act beginning to take effect on the same day.
>
> "As registrar of births, the dispensary medical officer was required, by the Act, to serve on the parents, when registering the birth of a child, a notice requiring them to bring it to him within six months to be vaccinated, and a notice stating the times and places appointed for the purpose. At the expiration of that time, if default had been made by the parents, the public vaccinator had ready means at hand for verifying the fact, &c."

Alluding to another point of great importance the Report continues thus:

> "In conclusion, we believe that the prosecutions of inoculators for smallpox, and the exemplary sentences which they have received during the last four years, have been of the utmost service in producing the apparently satisfactory state of things at which we have in so short a time arrived."

In reviewing the report of last year, we quoted an inquiry carried out by one of the English Poor-Law inspectors, who therein alluded to the subject of the inadequacy of the remuneration[1] received by the Irish Poor-Law medical officer; as yet his position has not been improved, although additional onerous, responsible, and dangerous duty has been inflicted upon him, viz. the examination of lunatics, the 10th section, Act 30 & 31 Vic., c. 118, distinctly providing that this most disagreeable duty, involving such risk, loss of time, and responsibility, shall be performed "without fee or reward."

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[1] As an instance of the inadequacy of the remuneration received by poor law medical officers, we may mention the case of Dr. Suffield, late medical officer to the Clifden Union, County Galway, who for twenty-seven years discharged the onerous duties of medical officer in that district, to the utmost satisfaction of the guardians and public generally, for £60 a year, and who is now paralysed and unable to leave his bed. He was refused a pension of even £40 a year, though the guardians repeatedly applied in his behalf, the poor law commissioners stating that on account of his having taken private practice he did not come within the law that enabled them to grant pensions.

Dr. Suffield was one of those whose public service extended over some of the darkest periods of Irish history, living through the famine years and the period when the awful fever that followed that time of destitution, when one medical man in every fifteen died of fever throughout Ireland in one year, and he distinguished himself at those times as well as during the last three visitations of cholera; and now he is denied a pittance, though his illness has been mainly brought on by his exertions in behalf of the sick and destitute during the long period of his public service.

It is, however, to be hoped, that the valuable services rendered to the State by the indefatigable and intelligent dispensary medical officers of Ireland, will not be much longer overlooked. The justice of granting improved pay to the medical officers is the more evident as another duty, of late expected of him, has been that of sanitary officer, and the strongest evidence of his efficiency is afforded by the remarkable diminution of smallpox, and the comparatively small mortality from cholera, fever, &c.

Up to the present time the medical inspection of the dispensaries and the dispensary districts has been conducted by medical men, and with what excellent results we have seen above. In the present year, however, *An Act to extend the Powers of Poor-Law Inspectors and Medical Inspectors in Ireland* (31*st July*, 1868) was passed to enable inspectors not having the qualification of physician or surgeon to assist in carrying out the provisions of the "said Act" as fully and as effectually as if he was a qualified medical practitioner. It seems an unusual proceeding to pass an Act of Parliament conferring the powers of carrying out the duties of a medical inspector upon a gentleman (not possessed of a medical education) as "fully and as effectually as if he was a qualified medical practitioner."

What will our professional brethren say to legislation such as that; those who, of late years, have had to register their qualifications, and pay heavily to a government body for their rights as medical practitioners? The question naturally arises, what are our privileges if those who may never have devoted their thoughts for one moment to the great medical questions, which must come under the notice of every one filling the office of medical inspector, are empowered to hold medical inquiries into the best means of alleviating the severity of, or, in the language of the day, "stamping out" the cholera, typhus, or other disorder; or of holding an inquiry into the treatment of epidemic meningitis, or into the correctness of the line of treatment pursued in any particular case of a surgical, obstetrical, or purely medical nature?

Who will believe that gentlemen acting as medical inspectors, solely by virtue of an Act of Parliament, are now to sit in judgment upon the prescriptions and treatment, in all its varied details, of cases occurring in the practice of the Irish Poor-Law medical service. If the system and machinery of that service had been found wanting in the hour of trial; and if, instead of being looked to as a model upon which the English Poor-Law authorities have founded certain alterations in their machinery,—that of appointing medical men as inspectors being a part of such alterations,—and if, instead of being viewed with confidence by the people of Ireland, and of serving as one great means in alleviating the sufferings of the Irish poor, in distress and in sickness, and of being, under Providence, the great means of almost banishing smallpox, and of materially lessening the fatal effects of cholera and fever, then we should have said the whole system required reform. But when the reverse of all this is the case, we cannot see why that which has worked well for many years should be altered.

We do not hesitate to call public attention to this matter, which appears to have been made law, without having been fully under the attention of either the Parliament or the Government.

The Irish people have judgment to perceive the value of medical science, as seen in their willingness to comply with the requisitions of the Compulsory Vaccination Act; we would assert for them, therefore, the right to continue to have the benefit of all that medical men, both as Poor-Law executive medical officers and as medical inspectors, can afford them, and we should rejoice in being able to claim for the whole of the people of England the same intelligent acquiescence with a law, so replete with good as the Compulsory Vaccination Act, as that afforded by our fellow-subjects in Ireland.

Art. V.—*Address on Health*, delivered at the Congress of the National Association for the Promotion of Social Science, held at Birmingham, October 1868. By Henry W. Rumsey, M.D., President of the Health Department of the Association.

No one can refuse to Dr. Rumsey the credit of earnestness—some would go to the extent of calling him an enthusiast—whatever the opinion that might be formed of his schemes for sanitary and sanatory administrative reform. These may be practicable or they may be Utopian. However it may be, there is this to be said, that each of us who has lived to middle age has lived to see a wondrous revolution both in public sentiment and in parliamentary efficiency in regard to questions of health and education, which, if not formerly wholly disregarded, were thrown into the shade by matters of political interest of quite a different nature. We are not among those who would put down enthusiasm as a feeling destructive of all calm judgment, with the well known injunction of the great diplomatist, "*Surtout, mon ami, point de zèle.*" On the contrary, it is from among the men who have exhibited this feeling most obviously, that we can pick out those who have been the originators of some of the most valuable institutions, political, social, or religious, which we possess. Dr. Rumsey's book on 'State Medicine' has been now some years in the hands of the public, and the practical verdict passed upon it has been that his views upon sanitary reform are in advance of the times. That is all. Public opinion and our legislators are not yet educated up to his mark, in addition to which radical reforms are not in accordance with the genius of the nation. The utmost that we can look for is to see our public arrangements modified piecemeal, without any particular thought being given to the fitting of one bit of legislation into another. The truth is, our sanitary legislative efforts have been a muddle of compromises, and a muddle we fear they will continue to be. *Le peuple le veut.* But it is no less an advantage to have in our possession a well thought out plan of sanitary organisation such as Dr. Rumsey has given us in his book, which we may place before our eyes as a pattern or model towards conformity with which we might do worse than strive. We are not quite sure that we cannot trace out in the present address some conviction of this kind as settling in the mind of the author himself. The Royal Commission, to the inquiries and labours of which Dr. Rumsey looks with hopefulness, may help forward the task of reducing to something like order our present confused mass of sanitary enactments; but above and beyond this is a House of Commons which, however politically constituted, will inevitably be steeped to the eyes in the social traditions of the country they represent. A Royal Commission may prove to demonstration what ought to be done to put our sanitary code into proper working order, but we cannot be sanguine that their most logical inferences will be put to a practical use when we call to mind the discussion on the Foreign Cattle Market Bill of last session, and the result which followed.

Now just let us look back and look around us for a minute. Are we not in a muddle, and in a somewhat disgraceful muddle, for a practical nation like ours? What do we see? First, in the words of Dr. Rumsey, "The main difficulty which meets the sanitary reformer is the existence of so many different kinds of local authority, in all manner of districts, for the execution of a variety of measures, which, if distinct from each other, are nevertheless cognate, and often strictly correlative." In one place it is the town council, in another the board of guardians, in a third it is improvement commissioners, in a fourth a vestry or board of works: and the worst of all is that one part of the sanitary work of a district is often entrusted to one authority, and another part of it to a different authority; so that no harmony can exist between them, and often the one authority utterly confounds the work attempted by the other. Secondly, the attempt to engraft new cloth upon an old garment, has terminated in its proverbial result: for whereas the common law provides a remedy of some sort for every evil under the sun, people have learned to neglect this, and look for their remedy under various Acts of Parliament at the hand of petty corporations composed of individuals who have a rooted repugnance to the task imposed upon them, who are often too ignorant to understand its importance, too stupid to learn it, and too much interested in the continuance of the present state of things, to take any trouble to alter it. Thirdly, we are suffering from the timidity of our legislators, who enact what have been termed "permissive" statutes when a bolder course was the only one likely to come to good. With all due respect to them, we must be allowed to say that, where grand social or sanitary reforms are needed, the passing of a permissive bill is nothing short of repudiation of the proper business of Parliament, and the shifting upon the local authority of the functions of the legislature. Who could ever expect a board, composed as most local boards are, for instance, to "adopt" —that is the word—the Adulteration of Food Act, or the Act for the Regulation of Workshops, or that men who never had the handling of five hundred pounds of their own would put in force such Acts as the Sanitary Act, or the Artisan's and Labourer's Dwelling Act of the last session? Of course, they don't see the use or necessity for them. Many of the persons who sit on such boards would be the first to suffer in their pockets were they to do so, and thus a fellow-feeling makes them wondrous cautious, lest they should interfere with private rights, although exercised to the endangerment and loss of of the community at large. Such sanitary legislation as this is nothing more substantial than moonshine. If we are to have local sanitary authorities—and we do not see how we are to do without them—the limits of the local jurisdictions should themselves be extended. Dr. Rumsey suggests "county" authority as a form of local authority in unison with our old traditions, and at the same time one which, by judicious improvement and reinforcement, might be readily adapted to our needs. Dr. Rumsey has not much faith in compulsory legislation, nor have we, so long as we continue to have it addressed to an ignorant executive whose ruling idea is the saving of the rates. The reform which we think chiefly needed, is in the constitution of the local authority in the first place; and, if we are to have in sanitary operations petty district corporations at all, they should form merely executive bodies to carry out to the satisfaction of the justices, or of the grand jury, public works which the local authority may decide to be necessary. In the execution of penal statutes, such as the Nuisances Removal Act, responsible paid officers, reporting to, and acting *spontaneously* under the sanction of the local authority, would be far more efficient than the three or four gentlemen who solemnly assemble at long intervals to determine whether an open foul drain is such a nuisance as to call for their interference for its immediate removal by process of law, or whether a dozen people shall be permitted to breed fever in a house by herding together in a room only fit to accommodate a third of the number.

It has been said of sermons that they ought to contain some grand central idea around which the observations of the preacher should revolve, and towards which they should all converge. And we say the same of addresses such as that before us. At any rate, we habitually look for such a central idea. If we have not "reviewed" Dr. Rumsey's address in the ordinary signification of that word, we have done something better, for we have, if we mistake not, picked out *his* central idea, in the light of which his entire discourse ought to be studied. There is a great deal in it which, if not new, is so true, that it cannot be repeated too often, or enforced too urgently. Dr. Rumsey has not spoken inconsiderately. No one knows better than he, what he is talking about, so that his address not only deserves to be read, but will be read. We have rarely perused a more comprehensive address.

ART. VI.—*On Asthma: its Pathology and Treatment.* By H. HYDE SALTER, M.D., F.R.S. Second Edition, 1868. Pp. 464.

ACCORDING to a well-established custom in the history of books, this second edition considerably exceeds its predecessor in bulk. Custom moreover requires not only additional matter, but also a declaration on the part of the author of the work, of the alterations, emendations and additions made; a requirement that must at least receive the commendation of reviewers who are called upon to reveal such particulars to their readers. Dutirully acquiescing in this practice, Dr. Salter tells us, in the preface to the second edition of his much valued work, that he has made few alterations in the views previously expressed by him in the first edition, and that the additions relate to therapeutics and largely consist in records of cases.

At this present period of medical literature, we may safely aver that additions of this character will be particularly appreciated by those actively engaged in the practice of their profession, who are frequently disappointed, when seeking for practical suggestions to aid them in the treatment of difficult and embarassing cases, to find, after reading most able and subtle disquisitions on pathology and morbid anatomy in modern medical treatises consulted, the therapeutics of disease dismissed in a few lines of unprofitable generalities. Those who are brought into contact with medical men who are not speculative thinkers only, but active practitioners, can confirm the existence of this prevailing complaint of the little value of most modern treatises on medicine as guides to treatment. How to detect disease is a thoroughly well-worked problem; but how to cure disease is one that has received too little attention from scientific physicians.

To return to Dr. Salter's account of his new matter. Much of this is made up of a notice of "the treatment of the asthmatic paroxysm by alcoholic stimulants; of the value of iodide of potassium as a remedy; of the therapeutical effects of chloroform," &c. He has "also added some interesting cases of uterine asthma, and asthma dependent on animal emanations. The number of tabulated cases has been raised from 44 to 223, and represents a mass of facts from which large and safe conclusions may be drawn, and which constitute (he remarks), in my opinion, one of the most valuable parts of the book."

Dr. Salter argues that asthma is intrinsically, and, with perhaps the exception of a single class of cases, exclusively a nervous disease; and that the nervous system is the seat of the essential pathological condition. Emphysema is the commonest product of asthma, and to explain its origin, the author adopts the hypothesis of emphysema propounded by Dr. Gairdner, admitting at the same time, with Dr. Walshe, that the connexion of that organic change with spasmodic seizures is certainly sometimes dependent on an intervening irritation or passive congestion of the tubes.

The consideration of the treatment of asthma occupies above 130 pages, and may consequently be pronounced adequate, and, we may add, complete. Why coffee gives relief, Dr. Salter would explain by reference to the particular nervous condition it produces—to the development of a state of mental activity and vivacity inimical to sleep,—sleep being favorable to the onset of the asthmatic paroxysm. Allowing this to be the *modus operandi* of coffee, it may also be assigned as that of strong tea, of alcohol and ammonia, of Indian hemp and ether, and other stimulants of undoubted value in asthma.

Of the merits of iodide of potassium as a remedy, the author speaks with faint praise. He finds it entirely fail in a great many cases, while those in which its success is complete are comparatively few.

The value of the various medicines, of hygienic conditions, of exercise and baths, of change of locality, &c., is discussed in detail, and numerous practical hints as to how and when to use particular remedies are interspersed. The narrative cases are, for the most part, related by the patients themselves, and constitute very interesting and instructive portraits of the disease. The tabulated cases reflect great credit upon the compiler; the number is so very large and the particulars collected so full and well arranged, that, to repeat the author's words, they "constitute one of the most valuable parts of the book," particularly to the student who would familiarise himself with the history of asthma and test the conclusions arrived at, whether by Dr. Salter or other writers. In conclusion, we have to restate the opinion of the great value of this treatise of asthma expressed in the notice of the first edition.

Art. VII.—*On Diseases of the Chest: being contributions to their Clinical History, Pathology and Treatment.* Part I. *Diseases of the Lungs;* Part II. *Diseases of the Heart and Thoracic Aneurism.* By A. T. H. Waters, M.D., &c., London and Liverpool, 1868. 8vo. Pp. 418.

In justice to the author it is necessary to copy the title in full, and also to place before the reader the purpose and scope of his work as set forth in the preface. For when we proceed to examine the contents of the volume, it is at once seen that it is far from being a complete treatise on Diseases of the Chest—pulmonary and cardiac. To quote the preface, the work is "but a series of contributions—chiefly in the form of lectures—to the clinical history, pathology and treatment of some of its more important affections." In fact, it is almost wholly made up of clinical lectures delivered to the pupils of the Northern Hospital, Liverpool, of a few communications to medical societies and journals, and of a republication of the author's well-known and valuable essays on the 'Anatomy of the Lung' and on 'Emphysema.' It may, in other words, be called an edition of collected works.

This statement of the nature of the treatise, though necessary on our part, should not be regarded as detracting in any measure from the value of its contents. To have a record of the views of such an experienced physician as Dr. Waters, and particularly in reference to the therapeutics of chest diseases,—a subject, we are happy to say, that occupies an important place in the several clinical lectures he has now published—is a desideratum.

The clinical lectures make up the bulk of the book, and are good examples of hospital teaching. Pneumonia is considered in a course of seven lectures, and the prominent position assigned in them to the subject of treatment will be indicated by quoting their several headings:—1, Pneumonia: its morbid anatomy and pathology; 2, its first morbid condition and earliest physical signs—cause of the crepitating râle, &c.; 3, treatment; 4, treatment—use of stimulants—cases; 5, complicated with delirium—cases—treatment; 6, occuring during rheumatic fever; 7, treatment—abstract of cases—pneumonia of children and of the aged; table of cases with abstract of treatment.

Those other diseases of the respiratory organs noticed in this treatise, have, however, not received the same amount of consideration. Œdema of the lungs and pulmonary apoplexy are discussed in one lecture; gangrene of the lung, phthisis with fetid expectoration and fetid bronchitis in another; pleurisy and pleuritic effusion in a third; and phthisis, its nature and treatment, in a fourth. This represents an inventory of the lung diseases, excepting emphysema —a subject made his own by a special treatise, here reproduced in six chapters—treated of by the author. The catalogue of heart-diseases also makes no pretence to completeness. It includes a lecture on the sounds of the heart; one on pericarditis; one on fatty disease; another on chronic disease of the valves and orifices, and on cardiac murmurs; and ends with a chapter on sudden death in relation to heart-disease, and on the probabilities of life in valvular diseases of the heart. To make the list of contents complete, we must note the reprint of a paper on "Asphyxia," from the 'Transactions of the Medico-Chirurgical Society,' in 1861, and of a short communication to 'The Lancet,' in 1861, on the "Rhythmical Action of the Heart"; and the introduction of a brief chapter on 'the Apnœa of Still-born Children'; of a record of cases of thoracic aneurysm with remarks, and, in conclusion, of a résumé of his views on the use of alcoholic stimulants.

To present a notice of a few of the opinions of the author concerning some debateable points; we may observe, in the first place, his agreement with the doctrines of most modern pathologists, of the absence of an interstitial tissue between the air-sacs, and of the presence of the exudation of pneumonia within those sacs. It is these sacs themselves that are inflamed, and the vessels concerned in the morbid process are those of the pulmonary plexus, derived from the pulmonary artery, and not the bronchial arteries. These conditions he regards as explaining the severity of the accompanying fever—like that of blood-disease, and the rapid consolidation of the lung. He concurs with Dr. Stokes in the existence of a stage of dryness and injection of the pulmonary membrane prior to engorgement, and of a corresponding early sign,—a loud, harsh respiratory murmur antecedent to crepitation, or the indication of engorgement. The normal respiratory murmer he places in the air-cells, and accounts for its production by a reference to the mode of connection of the bronchial ramification with the air-sacs, whereby an impediment to the passage of air into the latter is produced, and sound is the consequence. The puerile or harsh murmur of incipient pneumonia is but an exaggeration of the normal sound, and is the result of the dry and swollen condition of the pulmonary membrane; whilst the subsequent crepitation is due to the expansion of the sacs when their walls are covered with secretion or otherwise partially filled with fluid.

In the treatment of pneumonia Dr. Waters almost totally ignores bleeding, whether general or local, and rarely finds use for tartar emetic and calomel. Opium relieves the pain frequently present better than leeches or cupping, and ammonia and chloric ether, either with or without alcoholic stimulants, are oftener prescribed than small doses of antimony. The administration of salines he holds to be too frequently merely a matter of routine, and not necessarily indicated by the fever and hot skin, of which some form of alcohol will be ofttimes curative. In the stage of consolidation he has faith in the action of blisters.

The conclusions of the author respecting the relations between asthma and emphysema differ from those arrived at by Dr. Salter and others. Dr. Waters considers emphysema to be mostly the primary disease.

The lecture on phthisis is, apart from general rules for treatment, principally occupied with remarks on the climatic conditions of the disease—the tubercular form, however, being the only one referred to. At the conclusion of the chapter he notices the experiments of M. Villemin on inoculation, and observes that we need wait for further proof that the deposits produced were really tubercular.

In an appendix to his paper on Asphyxia, he expresses a preference of Marshall Hall's plan for performing artificial respiration over that of Dr. Sylvester, inasmuch as the former allows the fluid at the back of the mouth and in the air-tubes to be got rid of in cases of drowning. He would persevere with that method so long as there is an escape of fluids from the mouth and then resort to Sylvester's plan. In the apnœa of still-born infants, such methods of producing artificial respiration are inapplicable by themselves; because whilst in the asphyxiated adult respiration has to be restored, it has in the still-born infant to be primarily set up. Consequently, in the latter, a reflex act of respiration must be excited, as by cold or heat to the surface.

Dr. Waters treats somewhat at large of the causes of the sounds of the heart, and supports the hypotheses of Dr. Halford. He is convinced—

> "That the two sounds are simply and solely valvular; that the first results from the tension of the mitral and tricuspid valves, produced by the blood being forcibly propelled against them, and that the second is due to the tension of the semi-lunar valves of the aorta and pulmonary artery, produced at the time of their closure."—P. 302.

Neither in pleuritis nor in pericarditis has he faith in mercury.—

> "The only fatal case of acute pericarditis, uncomplicated with Bright's disease, which I have had amongst my hospital cases during the past eight years, was one in which I gave mercury to salivation. In this case, extensive effusion took place, which the mercury in no wise seemed to control."—P. 307.

In the matter of sudden death in valvular disease of the heart, he believes it to be a more likely event in connexion with mitral than with aortic regurgitation—contrary to Dr. Walshe's opinion. This belief he supports on theoretical grounds, observing especially the necessarily greater embarrassment of systemic circulation in mitral regurgitation. At the same time, he considers it—

> "A very rare thing for valvular disease to produce sudden death, unless the muscular substance of the heart has undergone a weakening or a degeneration of its fibre."—P. 380.

On the important subject of the use of alcoholic stimulants, he believes, that, from some years' experience, "speaking generally," their early administration "has reduced the rate of mortality of some diseases; and that, in a far larger number, it has diminished the intensity of the attacks, and accelerated the convalescence of the patient." Moreover, he holds, as a general principle, "that the therapeutic value of alcohol does not rest in its supposed use as an article of diet, nor on any supposed chemical change it may undergo" (p. 412). Amongst its most important advantages is its influence on the circulation, and this not only as a stimulant and tonic to the heart, but as a retarding agent in respect of the capillary circulation, whereby the abnormal development of inflammatory products is checked, and the wasting processes arrested.

But whilst advocating the therapeutical importance of alcohol, not in one disease specially, but in a large number of diseases, when its administration is guided, not by the name of the affection treated, but by the symptoms present,—he yet utters a warning against its indiscriminate or careless use.

The notice here presented of the contents of Dr. Waters' work, will suffice to show what important practical instruction it contains;—instruction founded on experience and illustrated at large by the record of cases. Nevertheless, as critics, we may take exception to the character of the book as a composition; seeing that, on one hand, it rather addresses itself by the amount of rudimentary teaching in the clinical lectures to students; and, on the other, is deficient in those lessons in minute and differential diagnosis, and in the general survey of lung and heart affections which students look for in treatises on diseases of the chest.

Art. VIII.—*A Dictionary of Chemistry.* By Henry Watts. In 5 volumes, pp. 5144. London. 1863—8.

Aided by fifteen coadjutors, many of them of high scientific position, Mr. Watts has completed most satisfactorily his great undertaking. It is a cyclopædia as well as a 'Dictionary of Chemistry.' It contains explanations of terms; it is also a collection of treatises on the various branches of the science.

We may here record our high estimate of the value of this Dictionary, without specially reviewing it. We may justly say that it worthily occupies an important place, hitherto unfilled, among books of reference. It is less diffuse and less historical than Gmelin's great work, but at the same time it includes many subjects connected with applied chemistry not touched upon in the bulky treatise of the German master. Again, it is written in the spirit of the new chemical philosophy, and its editor and contributors, appreciating the latest progress of the science, have not hesitated to change their notation, once at least, during the progress of the work. Apart from its fulness as a dictionary, wherein explanations of names and terms, with analyses and references, will be found, we are struck by its completeness as a series of essays on "analysis," "atomic weights," "volumetric analysis," "sulphur, acids, and salts," "arsenic," "quinine," "saliva," "urine and its analysis," "respiration," "bread," and a thousand and one other matters where chemistry is intimately bound up with arts, or the processes of common life. These essays are not all of equal merit or equal completeness, but they certainly constitute, taken together, the most full digest of all the recent chemical discoveries and advances, both as to fact and as to theory. A few less important paragraphs are rather behind the age, and below the standard of the book: these are signed with the letter U, and are parts of the original work, on which this book was founded: they are, in fact, the fossilized scientific remains of Dr. Ure.

ART. IX.—*The Natural and Morbid Changes of the Human Eye, and their Treatment.* By CHARLES BADER, Ophthalmic Assistant-Surgeon to Guy's Hospital. London. Pp. 505.

FEW English surgeons are better qualified than Mr. Bader to write a treatise on ophthalmic matters. He has studied his subject *con amore,* and for many years. He has enjoyed the widest opportunities of observation, and, latterly, of teaching also. He is by birth familiar with the language in which most of the discoveries in this department have of late years been announced. He was, perhaps, the first in England who became familiar with the use of the ophthalmoscope, and his pen assisted in the contribution (to our own pages) of the first paper of any value on this subject in our language. With such qualifications for his task, Mr. Bader's work has naturally excited great expectations. Nor will the patient reader of it be disappointed. The book before us gives proof of the possession of detailed knowledge in almost every part of the subject, and comprises a wonderful amount of information.

Mr. Bader tells us in his preface that he was induced to write by the need of some book of reference to which he could send his pupils. There is no doubt that the want of a work on ophthalmic subjects which should be up to the day, and should be neither too sketchy nor too encyclopædic, has long been felt. Mr. Dixon's little treatise, admirable as far as it goes, does not give nearly enough, and although well suited as "first lines" for a student, is of little service to one more advanced. Mackenzie's all-including treatise, marvellously accurate and complete, is now old, and was even when it held first rank, liable to the charge of being too diffuse. Whether, however, Mr. Bader has succeeded in supplying the want may be somewhat open to question. He has made a strenuous and praiseworthy endeavour. He has written a book which ought to be in the library of every surgeon practising in this department. We fear, however, that it is one to which Browning's line—

> "'Tis the taught already that profits by teaching"—

will be found especially applicable. Mr. Bader's style is not happy, and he has put himself into fetters which increase its peculiarities. Throughout the book he avoids entirely the mention of names, and shuns all forms of criticism, aiming to give in the fewest words possible the expression of his own present belief, without any reference whatever to the opinions of others. Now such a rule might do well enough for the compilation of a catechism intended to be committed to memory, but it is a very bad one for a long book which is designed to be read, and to be read with more or less of pleasure. It deprives the writer of even the small legitimate sources of dramatic effect which are still open to the scientific author. Apart from the question of justice to previous writers (not, we think, a trivial one*) the omission of names does an injury to the writer himself, and takes from his pages the flavour of humanity which is so agreeable to most of us in the midst of scientific work. Mr. Bader seems to believe that by offering his guests one dish only, and forbidding all conversation during dinner, he can make them eat more and digest better. We have our doubts, and whilst we thank him heartily for giving us

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[1] In connexion with the brief chapter on that most interesting malady, proptosis with bronchocele, it is a thousand pities that the student is not informed in a foot-note that he can find a splendid clinical account of it in a book now so accessible as Vol. I of Trousseau's Medicine. Mr. Bader's adherence to his rule is however absolute, and neither Graves, Basedow, Gräfe, nor Trousseau receive a word of recognition in respect to it.

roast beef of the best quality, we should have been glad if he had diversified the feast, and managed the accessories better.

A much graver fault of style than the one we have just referred to consists in the want of artistic construction of the paragraphs and chapters. The book reminds us of one of Hogarth's caricatures, in which the laws of perspective and of relative size are wholly neglected. The individual figures are well drawn, and there are plenty of them, but they are not "placed." The church, which ought to be on a distant hill, looks as if it stood on the parapet of the bridge close to us, and a crow just settling on a tree-top, is made as big as the tree itself.

Mr. Bader puts his statements into short definite sentences, and then, with but little regard to natural order, and with no respect to connexion, puts these into paragraphs to agree together as best they may. A person conversant with the subject can get at his meaning, and will usually find it worth his trouble; but to those who come *de novó* to the matter many statements will be incomprehensible and some misleading.

We are obliged, as honest critics, to point out these defects; they do not in the least affect our author as a surgeon, but merely as a writer for students. We now turn to a pleasanter part of our duty.

Preceding the discussion of the diseases of special structures, a brief anatomical description of the structure is always introduced. In this respect Mr. Bader and Mr. Power (whose work we noticed recently) both follow the excellent example of Stelwag von Carion. Mr. Bader's anatomical information is concise and very good; he often succeeds in connecting minute anatomy with pathology in a very instructive manner.

The chapters on operations strike us as written with great care and with an earnest desire to instruct. After a minute description of the various steps of each operation, Mr. Bader gives us an appendix on the "accidents" which may happen during its performance. These accounts of accidents are especially instructive, and show at once a skilled operator, and one who has observed extensively the practice of others. We know of nothing to compare with them in precision or detail in any other English work. Our space does not permit of quotations, or we should have liked to take parts of the directions for iridectomy in glaucoma, given at page 412 *et seq*. The whole account of glaucoma is excellent, if we make some little allowance for the peculiarities of style to which we have adverted. So also are the chapters which deal with defects of accommodation and refraction. These difficult subjects are very clearly explained, and the pages which deal with them are crowded with important practical hints. It is evidently a department to which Mr. Bader has given special attention.

We observe with pleasure throughout the book that the author, whilst he has no fear of being definite, carefully avoids narrowness. All his pathological teaching is wide. He puts no extreme faith in names, or in subdivisions. We do not find diseases cut, dried, and labelled, like flowers in an herbarium, as if each one was certain to be an exact specimen of all others of its class. In a few instances we have thought, in reading his book, that he might have been more precise with advantage, but even here, if wrong, he errs on the right side.

Under the head of "*Anomalies of Vision*," at page 439, Mr. Bader gives a short recapitulation of "the numerous terms which are made use of by patients to designate the different kinds of impaired vision." An explanation of these several terms is appended, or a reference given to some other part of the book where such explanation may be found. These will, doubtless, be found very useful.

The work does not impress us as of equal excellence in all parts, and, on the whole, we like its first half less than the latter one. This may, perhaps, have been from the fact that some of the subjects have claimed the author's interest more strongly than others. The chapters on ophthalmia, on iritis, and on cognate subjects, are not nearly so full and clear as those to which we have just adverted.

We must notice a few defects, either of expression or of fact. In doing so, we must assure both author and reader that we should not take the trouble to point out a few faults, did we not very highly value the work as a whole. At page 232 we find, "The nearer to the eyes an object is held, the more do the rays diverge which come from the object." This expression surely is incorrect. The nearer to the eye the object is held the greater is the number of rays from it which will enter the pupil, and of these, the peripheral ones will, of course, be increasingly divergent. In relation to the object, the rays which come from it keep exactly their original places, and diverge neither more nor less. The expression is not only incorrect, it is misleading, and we are not any the more disposed to pass it over on the present occasion because it is used, as we are aware, in several other educational books. Whether or not it has misled, may be ascertained by asking a student to try the simple experiment of reading small type at two inches through a pin-hole aperture, and to explain the action of the card.

"*Glaucomatous Retinæ*" is, again, an expression which, occurring in a students' handbook, deserves no mercy. The initiated know well enough that Mr. Bader means simply retinæ from glaucomatous eyes, but a student would be obliged to suppose that glaucoma is a disease of the retina. "*Dissections of numerous glaucomatous retinæ removed from the living body*" ought, again, to read simply—Dissections of the retinæ from numerous glaucomatous eyes excised during life.

"Alterations in elasticity and curvature of light-refracting surfaces." Surely it is the substance, and not the "surface" which is elastic, and although the expression—"light-refracting surfaces" may, in a certain sense be defended, it is not a usual one, and is likely to puzzle a student.

We have for long been familiar with acne tubercles, and acne pustules, but what on earth is an "acne node"? (page 40).

Rodent cancer, the "peculiar ulcer of the eyelids" of Jacob, a form of disease which has been very accurately described by several recent writers, receives at the hands of Dr. Bader but very imperfect notice. He mentions it only as "epithelial cancer," and says not a word of the peculiar features which Dr. Jacob, Brodie, Paget, and others, have so well pointed out. His reader might suppose, for anything that is stated to the contrary, that "epithelial cancer of the eyelids" is just the same disease as epithelial cancer of the lips. Now, whilst fully admitting that rodent cancer is no doubt a variety of "epithelial," or rather "epidermic" cancer, yet its clinical differences are most important. The one destroys life in as many months as the other takes years; the one quickly and certainly causes implication of the lymphatic glands, the other does so with extreme rarity. We scarcely know what Mr. Bader refers to, when he says: "The cancer reappears more speedily if the operation be performed after the glands have become implicated." *Never* is a strong word, but we might almost use it, and declare that in the rodent (or epithelial) cancer of the eyelids the glands never do become enlarged. At any rate, their immunity is a fact so constant and remarkable that it ought not to have been ignored, as it is in the sentence we have quoted, and in the rest of the description.

The omission of names, to which we have already referred, is also throughout the book a very serious inconvenience. We cannot tell what assertions are made on the author's own authority, and what are quoted; nor respecting the latter can we, if we wished, refer to the original. For example, at page 403 occurs the following sentence: "In a case (a patient?), aged seven, simple glaucoma of both eyes; and in another, aged ten, acute glaucoma of both eyes, occurred." Now, those who are sceptical about glaucoma in the young, and who know how many mistakes are made with the ophthalmoscope, and how many mistaken observations find their way into print, would like to know the authorities for these cases. If Mr. Bader told us that the cases were his own, or that they were recorded by Græfe, Donders, Liebreich, or Bowman, all further investigation is made unnecessary, whilst if the name should be an unknown one we might incline to turn up the original record, and see if it were convincing. Left, however, in total ignorance as to the who and the where, we neither venture to believe nor to doubt, and have no chance of gaining a solution of our difficulty.

Mr. Bader's reference to tobacco-amaurosis is very brief, but we gather that he is a believer in the supposed cause. We doubt his correctness when he alludes to "basilar meningitis" as occurring in these cases.

We find detailed chapters on "milium of the eyelids" and "acne of the eyelashes," neither of them affections of any importance, and the latter very doubtfully distinct from the common ophthalmia tarsi. Yet there is no mention of herpes ophthalmicus (or herpes frontalis), a disease of the utmost interest to the pathologist, and of considerable importance even to the practitioner.

As regards the getting up of the book, we may say, that excepting that the page is much too large, the paper, type, &c., are everything that could be wished. With the volume of letterpress, but sold separately, is a collection of plates, chiefly from the ophthalmoscope. These are enclosed in a small and very neat portfolio. Many of the plates are good and others tolerable, whilst some are only poor. There are a few which it would puzzle the most skilled ophthalmoscopist to recognise. Liebreich's 'Atlas' has, however, spoiled us for poor plates on this subject, and knowing something of the difficulties in getting lithographic artists to work satisfactorily, we sincerely sympathize with Mr. Bader in the disappointment which some of his must have caused. The plain lithographic plates, instruments, sections of the eyeball, or retina, syphilitic teeth, &c., would have been far better given in the text as woodcuts. Some of these are good, but we cannot praise them all. Those which show the deformities of teeth characteristic of inherited syphilis, strike us as very bad, and very likely to lead to error. We doubt whether even Mr. Hutchinson would recognise them. That they should be as they are is the more pity, because very good delineations have been given by Mr. Erichsen, Mr. Dixon, and Dr. Williams, of New York, not to mention the originals.

If, in concluding this review, we again assure Mr. Bader that our estimate of his work is very high, we fear he may incline to compare our praise to that of the critic who told his friend that his picture was excellent, and that it only needed to have the principal figures done afresh, and a new background put in. But really such is not our opinion. The principal figures, the facts, are excellent; it is the background chiefly that wants improvement. In his next edition, which will, we hope, soon be necessary, we shall trust to see names and references judiciously introduced, and good woodcuts freely interspersed with the text. By attention to these points, and a few other little matters, the work may easily take a high place in medical literature.

ART. X. *Des Grandes Epidémies, et de leur prophylaxie internationale, avec le texte des Lois, Decrets, Arretes, Ordonnances et Instructions qui s'y rattachent.* Par LEON DEPAUTAINE, M.D. Paris, 1868. 8vo. Pp. 194.

Of the Great Epidemics, and of their International Prophylaxis, with the text of the Laws, Decrees, &c., relating to them. By LEON DEPAUTAINE. M.D.

EPIDEMIOLOGICAL investigations generally, and especially those relating to the important subject of the prophylaxis of certain epidemic diseases, and the prevention of their spread from one land to another, continue to excite the liveliest interest in France. It is surprising, and not a little to be regretted, that comparatively so little has been done by British physicians in this field of research; and that a country like ours, which is, in truth, more directly concerned than any other in a rational and rightful solution of the many difficult questions involved in the inquiry, and which moreover possesses the best opportunities for investigating them in all their complexities, should lag so far behind in appreciating the importance of this branch of State medicine. It is to France that we owe the suggestion of the two international sanitary conferences in 1851 and 1866, the mere origination of which, whatever may be thought of the practical value of the labours of these commissions, is sufficient to attest the enlightened zeal of the Emperor's government in such matters. Nor should we forget the wise measure, that was adopted in the time of Louis Philippe, of appointing five or six medical men to be resident or sanitary physicians in the principal cities of the Turkish empire, a step which has unquestionably been productive of very great benefit to the East.

The present work consists of two parts. The first section consists of miscellaneous observations and extracts concerning the plague, yellow fever, and cholera—the great epidemics—and the second consists of a reprint of all the laws and ordinances relating to quarantine in France which have been enacted during the last century and a half, and down to the present time. The following conclusions contain the pith of the first part:—

1. The Oriental plague, the cholera, and the yellow fever exist in the endemic state in the three deltas respectively of the Nile, the Ganges, and the Mississippi.

2. Their common and primordial origin may be ascribed to marsh exhalations, or, in other words, to infection.

3. They may, each, become epidemic and contagious.

4. Quarantine, and other measures hitherto employed as means of preservation against the invasion of epidemic visitations, can only be regarded as temporary or occasional expedients.

5. The penalty of death imposed by the French law of March 9, 1822, for violation of quarantine regulations in respect of foul bills of health, and still unrepealed, ought to be abolished from our code.

6. The different governments of Europe, and that of the United States, ought to combine for the purpose of extinguishing on the spot the plague, the yellow fever, and the cholera in the principal sources of their origin.

7. A permanent commission composed of competent men, as delegates from the different governments, should be invested with all due powers for carrying out so important a work.

8. Established on the shores of the Suez canal, under the common guarantee of all the Powers, this commission would maintain a surveillance over all arrivals at Egyptian ports, and departures therefrom; and be enabled to indicate to the government of each nation any impending or apprehended epidemic, and the proper means of prophylaxis to be adopted.

These propositions and recommendations are, of course, open to controversy in several particulars, scientific as well as practical. They exhibit the present French view of the subject.

The following extract, relating to a recent ordinance respecting quarantine regulations in France, is from an official report which was addressed to the Emperor by the Ministers of Foreign Affairs and of Commerce shortly before the publication of the Report of the Constantinople Conference, reviewed in the July number of this journal:—

"We are glad to report that this year (1866), the sanitary state of the pilgrims to Mecca has been very satisfactory, and that their congregation in the Holy Cities has not been attended with any malady of a threatening nature to the countries which they had to traverse on their return. Various causes contributed to this result. On the one hand, the caravans were less numerous and crowded; and, moreover, the sanitary measures which had been put in force, under the energetic surveillance of the Grand Scherif, ever since the preceding year, must have been of the greatest utility. Thus, all the sacrifices of animals were performed outside the city of Mecca, and in suitable localities provided for the purpose, and the remnants of the slaughtered animals were buried in deep pits. Proper precautions were also taken to maintain personal cleanliness among the pilgrims, and remove all accumulations of ordure. The town of Djeddah has undergone a thorough transformation; the streets have been purified, the houses renewed, and the old foul magazines in the bazaar have been replaced by new and commodious buildings. These important improvements were carried out in accordance with the urgent advice of the International Conference, which found a zealous exponent of their views in the late Dr. Schnepp, Sanitary Physician and Vice-consul of France at Djeddah, who fell a victim at his post in the autumn of last year.

"The sanitary commission that was sent by the Turkish government has kept up an active surveillance, in concert with Dr. Schnepp's successor at Djeddah, over the vessels with pilgrims on board which arrive at or leave the port of Djeddah, so as to prevent undue crowding, and the neglect of necessary precautions. One result of this measure has been that a larger proportion of the pilgrims returned to Syria by land, than in the former year."

MM. Moustier and Fourcade will not commit themselves, or the Emperor's government, to the adoption of the International Conference's conclusions on the subject of quarantine in respect of the cholera:—

"We are not ignorant, sire, that this question is still too much controverted for the opinions expressed by the conference assembled at Constantinople, however valuable their labours may be, not to encounter serious objections. Therefore, we shall guard ourselves from prejudging a solution which, as it seems to us, can only be arrived at after new inquiries, and after a thorough critical examination of the diverse doctrines still in vogue. We limit our remarks to merely mentioning this part of the Commission's researches, reserving, in so delicate a matter, perfect freedom of action to your Majesty's government."

We have not heard whether the French Government propose to adopt any practical alterations in their quarantine regulations, since these remarks were written in 1866.

ART. XI.—*A Collection of the Published Writings of the late* THOMAS ADDISON, M.D., Physician to Guy's Hospital. Edited, with introductory prefaces, by Dr. Wilks and Dr. Daldy. New Sydenham Society, London. 1868. Pp. 242.

THIS volume will be gratefully received by the members of the Sydenham Society, indeed by the whole profession. Its value is enhanced by the introduction of illustrations, among which are three well coloured plates elucidating Dr. Addison's "Observations on Pneumonia and its consequences," especially introduced by the editors on account of the importance of the subject, of the views enunciated, and of the period at which those views were advanced. As there are no references to these coloured lithographs in the text, we presume, in the absence of any statement, the editors have selected the morbid specimens and superintended their engraving, as fitting exemplifications of Addison's views. Nevertheless, in the preface, the editors state that there are, in the museum of Guy's Hospital, numerous drawings made by Addison himself from his own preparations, exhibiting the forms of disease grouped under the names of "phthisis and pneumonia," and almost every variety of chronic pulmonary disease to be met with. These drawings would, the editors observe, have constituted a most valuable contribution to the knowledge of such disease, and they have to lament that the funds of the Society and the space allotted them in the published volume would not admit of their introduction. The regret will be shared by every student; for the publication of well-engraved coloured plates illustrative of morbid anatomy, copied from drawings of well chosen preparations, is a want in the British medical book market, and probably can be supplied only by such an agency as the Sydenham Society. We commend the consideration of this suggestion to the council of the society.

But to return to the book before us. The prefaces introduced by the editors give a sketch of the purpose and teachings of the several contributions of Dr. Addison. The object of the publication, they remark, is "to inculcate peculiarly the doctrines of Addison." The greater part of the volume is occupied by various papers read by him from time to time at one or other medical society of the metropolis, on diseases of the lungs, wherein he enunciated propositions and pathological deductions in contrariety to the prevalent opinions of the period, but which in the course of subsequent research have become generally recognised as true. The Council of the Society were particularly induced to print these papers, however, because the views of Dr. Addison on pneumonia and phthisis have

> "Not yet received due consideration; for even now the term phthisis, implying simply a state of lung prone to disorganisation, is regarded by too many of our profession as having always a basis of tubercle, and a consequent uniform mode of treatment is adopted, having commonly for its object a combat with this imaginary deposit."—P. xx.

Addison was one of the first to demolish the hypothesis of a generally present pulmonary parenchyma, of connective tissue disposed about every air-cell and being the seat of disease. The first paper in the present collection of his writings is devoted to the anatomy of the lungs, and especially to show that all the great anatomists had taught erroneously that the pulmonary arteries and veins were throughout the lung contiguous, whereas, in truth, the vein pursues its course alone, outside the aerial tissue, whilst the artery ramifies within it. In the same essay, read in 1840, he laid down ten propositions, most of which were subsequently examined in future communications. Prominent among them were the assertions that pneumonia consists essentially in inflammation of the aerial cells; that pneumonia and inflammatory tubercle are identical, and that emphysema consists chiefly of a mere dilatation of the cells, but in part also sometimes of more or less extensive laceration of them.

The editors rightly observe that in the lapse of a quarter of a century much of the teaching which constituted the originality of Addison's essays has become absorbed into general medical literature; but still those essays possess an intrinsic value for the cogency and clearness of their matter, as well as a historic importance in English medical literature. He was one of the first exponents in England of the discoveries and doctrines of the illustrious Laennec respecting chest exploration and chest diseases; and at the time of its production, the second paper in the present collection, on the "Diagnosis of Pneumonia," must have exercised great influence in promoting the acceptance of those discoveries and doctrines, though to the student of the present day, the truths therein enunciated, are among the commonplace facts in pulmonary pathology. The like may be asserted of the essay on "Physical Diagnosis in Diseases of the Chest;" yet, here again, as the editors remark, the forty-two propositions laid down in that paper, "are as true, as urgent, as applicable to the emergencies of daily practice *now*, as when they were published twenty-two years ago." P. xxix.

But besides the essays on chest disease, which occupied Addison's attentive study for many years, the editors have re-published several papers on other subjects, including the celebrated essay "On the Constitutional and Local Effects of Disease of the Supra-renal Capsules," which appeared in 1855, and on which so much of his fame rests. This requires no notice on our part, and with reference to the other memoirs, we shall content ourselves with an enumeration of them by title: "Observations on Fatty Degeneration of the Liver;" "On the Disorders of Females connected with Uterine Irritation;" "Case of Ovarian Dropsy removed by the accidental Rupture of the Cyst;" "On a certain Affection of the Skin, Vitiligoidea plana and tuberosa;" "On the Keloid of Alibert and on true Keloid;" "On the Disorders of the Brain connected with diseased Kidneys;" and "On the Influence of Electricity as a remedy in certain Convulsive and Spasmodic Diseases." It is superfluous to say, that the perusal of any or all of these papers will prove them to be "the outcome of sterling, honest reflection upon carefully reduced facts," and of lasting value to the student of medicine.

ART. XII.—*Vesico-vaginal Fistula, from Parturition and other causes; with cases of Recto-vaginal Fistula.* By THOMAS ADDIS EMMET, M.D., Surgeon-in-Chief to the New York State Woman's Hospital, &c. New York, 1868. Pp. 250.

THIS volume is full of interest and information on all points connected with the distressing malady to which it refers. Dr. Emmet was a pupil and assistant of Dr. Marion Sims; and after witnessing that surgeon's practice, he has had a most ample experience of his own. The extent of this experience may be judged from the fact that Dr. Emmet's public and private practice has given him, for the last five years, an average of two such operations every week during nine months of the year. He accounts for this enormous number of cases of this description, by explaining that patients are received at the hospital from all parts of the country, and that many of them are women of the poorest class who had not received proper attendance, or who came from the frontier, or thinly settled parts of the country. The success which has attended his treatment is thus stated:—

"Previous to October, 1867, I had had about 270 cases under my charge. About 200 had been cured, five I had regarded as incurable, and between fifty and sixty returned home improved, the greater proportion of which will be cured if they ever return."—P. 15.

Dr. Emmet states that of all the cases of vesico-vaginal fistula admitted into the hospital during the twelve years which had elapsed since its foundation, he could not satisfy himself that more than three should be regarded as having resulted from instrumental delivery—a strong proof, to his mind, of the great danger of the accepted teaching in midwifery, "to wait any given length of time after the occurrence of impaction, with the hope that nature may yet accomplish the delivery unaided."

Dr. Emmet states that the operation is a most easy one when properly understood, requires no special operative talent or peculiar education, need by no means be confined to any special class of surgeons, and, in a great proportion of cases, is almost sure of success; those, that is to say, in which the parts are ample in quantity, and free from cicatrisation; so that healthy tissues can be brought together without traction.

Our author's directions for the performance of the operation are minute and precise, and he appends drawings of all the instruments required, but we need not go minutely into this matter. Thanks, in great part, to the teaching of American surgeons, this operation is now perfectly well understood, and practised with great success in this country; and although there are some few peculiarities in Dr. Sims's method (followed by Dr. Emmet) which are not usually imitated in this country, it does not seem clear that they contribute much to the great success of these surgeons. More, no doubt, is due to the perfect familiarity with every detail of the operation, which is obtained by constant practice than by minute differences in instruments, and in the method of passing sutures. It appears to be assumed throughout the book that chloroform is not administered. If so, we should be hardly disposed to adopt this part of Dr. Sims's practice, since the same success can in general be attained, and the patient saved the pain and shame of the proceeding. We would just call our reader's attention to Dr. Emmet's preference for the scissors over the knife, both in paring the edges, and still more in the preliminary section of cicatrices and bands of adhesion so frequently necessary in these cases. After such division and laceration with the fingers, Dr. Emmet speaks highly of the advantage obtained by dilating the vagina with a glass plug, which restrains the hæmorrhage and maintains the natural size of the canal. But the chief value of Dr. Emmet's work consists in the ample information it contains on the treatment of the more complicated and difficult cases—such as where the vagina has been closed by sloughing, and the os uteri is concealed; where the whole of the base of the bladder has been lost; where the urethra has become obliterated; where the laceration has extended into the rectum, &c.

In these apparently desperate circumstances Dr. Emmet shows how much may be accomplished by patience, in the gradual removal of the obstacles which successively present themselves; until, if the confidence of the patient is not worn out by repeated apparent failure, she is in many cases restored to comfort, and sometimes the functions are completely regained. The detailed notes of seventy-five cases of various degrees of complication, point out the course to be pursued in almost every conceivable emergency. They are, many of them, of great individual interest. We may notice one, page 221, in which the fistula was caused by a gunshot wound; and another, page 242, in which the patient, being very unruly, the peritoneal cavity was accidentally opened freely, so that air rushed into and out of it, notwithstanding which no untoward symptoms ensued, and the operation was perfectly successful.

ART. XIII.—*De la Kélotomie sans réduction. Nouvelle méthode opératoire de la Hernie étranglée.* Par le Dr. MARC GIRARD, &c. Paris, 1866. Pp. 276.

On Herniotomy without reduction. A new method of operation for strangulated Hernia. By Dr. M. GIRARD.

THE object of this work is to prove that the cause of the great mortality known to follow the operation of strangulated hernia, is the reduction of inflamed bowel within the peritoneal cavity; and the inference drawn by the author is, that the best course to pursue is merely to incise the stricture, and leave the gut in the sac.

The proposal is not so revolutionary as perhaps some of our readers may at first sight imagine. Mr. Jonathan Hutchinson, in an able memoir "On the Chief Cause of Death after Hernia Operations," published in the 2nd volume of the 'London Hospital Reports,' has endeavoured to prove pretty nearly what Dr. M. Girard lays down as the basis of his theory; viz., that it is the reduction of the intestine which commonly gives rise to peritonitis; and he proposes, as our author does, to meet the danger by leaving the intestine in the sac, *i.e.* unreduced, though he only applies the proposal to cases in which the operator has ascertained the existence of distinct morbid lesions in the herniated intestine.

Let us follow Dr. Girard (as well as extreme condensation will allow) through the successive steps of his argument.

In the first place he has, unfortunately, no difficulty in proving the great mortality which in all parts of the world follows the operation of herniotomy. He refers to published authorities for 1525 cases, 794 of which, or more than one-half, died. Still, the results of the operations which Dr. Girard has himself seen, were peculiarly unfortunate, and no doubt have given him an exaggeratedly unfavourable idea of the results of ordinary practice. He seems never to have himself operated for hernia; but in the course of his studies at Bordeaux, has witnessed fifteen operations, followed in eleven cases by death, directly due to the operation, and in a twelfth case by chornic peritonitis, terminating fatally. We may also observe that the general statistical data above given are considerably above the average mortality of institutions where no special unfavourable influence is acting, and where the rule of early operation prevails. Thus, at St. George's Hospital, where the cases seem to be received in a less desperate condition than in hospitals situated in the poorest parts of London, the mortality is only about one in three.[1] Still, there can be no question that the mortality after herniotomy is very high.

Dr. Girard next addresses himself to the question, Is this mortality caused by the disease or by the operation? He conclude that it must be laid to the charge of the operation, because peritonitis, which is the common cause of death, does not usually exist until after the operation has been performed. Here, again, we think that Dr. Girard rather exaggerates the infrequency of peritonitis as a result of mere strangulation, apart from any operation; yet we are quite willing to concede that peritonitis does very commonly set in after the operation.

Having thus laid down the basis of his argument, Dr. Girard proceeds, with much clearness and logical precision, to divide the operation itself into its four stages:—1. Incision of the superficial parts; 2. Opening of the sac; 3. Incision of the stricture; and 4. Reduction of the herniated viscera. The superficial incisions can have no influence on the mortality, except in very rare instances. With regard to the operation without opening the sac, Dr. Girard is an indiscriminate, and, we can hardly avoid saying, not a very well informed opponent of this proceeding. He speaks of it, in fact, with contempt. "There are yet," he says, "a few surgeons here and there (*quelques rares chirurgiens*) who liberate the strangulated parts, and reduce the intestine without opening the sac" (p. 17).

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[1] 'St. George's Hospital Reports,' vol. ii, p. 307.

Then he proceeds to show, as he believes, the absurdity of the practice. But a more extended knowledge of the subject might have taught him that though very unadvisable if indiscriminately applied, this operation is supported by a very great number of the best authorities, is largely practised (we should have believed in France, certainly in England, America, and Germany), and has led to the best results when temperately and judiciously followed.[1] Still, we are quite willing to concede to Dr. Girard that in the great majority of cases it is better to open the sac, and that such opening cannot be proved to produce any bad result. With regard to the incision into the stricture, Dr. Girard points out that during strangulation the hernial sac cannot strictly be said to form part of the peritoneal cavity—a proposition which he emits with some hesitation, as savouring of paradox—but which is sufficiently familiar to surgeons in this country. He also shows well enough that the risks of this incision (from possible wounds of the gut or of some artery) ought not to deter the surgeon from so necessary a part of the proceeding—and, besides, are in themselves not formidable. But he does not draw the inference which appears to us inevitable from his own premises, viz. that when once the stricture has been relieved, and the circulation permitted to go on freely between the herniated bowel, or omentum, and the viscera with which it is continuous, this separation between the sac and the general peritoneal cavity ceases, and morbid actions may be as freely propagated from one part of the intestine to the other, when left unreduced as if it were in the belly. We must say that to have passed over so obvious a consideration without notice, is a grave flaw in Dr. Girard's logic. Surely the analogy of internal strangulation, which so frequently exists for long periods of time without peritonitis, will show that the reason why the neighbouring intestines are not inflamed in most cases, is because the gut is too tightly constricted, as in Travers's experiment of tying a ligature tightly on the intestine of a dog, which produced no peritonitis of the gut around; and that there is just as much theoretical reason for expecting that peritonitis should follow on the non-reduction as on the reduction of the herniated bowel, after the division of the stricture; allowing that reduction may be effected without injury to the intestine. Dr. Girard dwells much on the injury done to the bowel in the manipulation necessary to reduce it; and we should almost fear, from the reports of cases which follow, that the surgeons whose practice he has witnessed, were hardly enough impressed with the necessity of gentle manipulation. As a general rule, no force is required to reduce a hernia after the fair and full division of the stricture. In voluminous hernia, indeed, much embarrassment and trouble is experienced, and much mischief is often done. So, also, if the gut is already nearly perforated by ulceration, or is much softened by inflammation, it may be ruptured even by gentle handling; and in such cases it is a very fair matter of question whether it is better to reduce the gut or to leave it in the sac. But looking at ordinary cases of hernia, we can see no reason whatever to admit Dr. Girard's proposition thus expressed:

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[1] In the paper above referred to, p. 323, reference is made to fourteen cases, operated on at St George's Hospital, without incision of the sac—nine inguinal, and five femoral, of which only one died, and that from causes quite unconnected with the method of operation, viz., diffuse inflammation round the wound.

> "Reduction is an irrational manœuvre, dangerous and in most cases fatal to the subject of the operation—incontestable utility could alone make it legitimate. Now no such utility exists,—this is shown by facts in the plainest possible manner. Therefore, the intestine ought not to be reduced; it ought to be left in the wound, and the fourth step of the operation of herniotomy ought to be entirely suppressed."—P. 204.

Dr. Girard gives, indeed, a number of cases, very interesting and valuable, in which the gut has been left unreduced, on account of various morbid conditions which rendered it, in the judgment of the operator, either necessary or, at any rate, advisable; and this is, to our minds, the most valuable part of Dr. Girard's work. Not, indeed, that Dr. Girard need have proved—as he takes great pains to do—that reduction is not necessary in order to procure the passage of fæcal matter through the tube of intestine. If any person held such an opinion (which the mere observation of everyday cases of irreducible hernia might have refuted), it is not one of the grounds on which surgeons of the present day consider reduction to be the proper course to pursue in herniotomy, speaking generally. Those grounds we believe to be as follows, and we do not see that they are at all sufficiently appreciated in Dr. Girard's book. First. That in all ordinary cases reduction does no harm. This, we allow, Dr. Girard discusses throughout his work, and thinks that he disproves; but we still maintain that he confounds the effect of undue violence in reduction with the natural and necessary consequences of the gentle replacement of the intestine. For ourselves, we must confess that we should regard a surgeon as almost beside himself, who, having before him a little knuckle of healthy gut, just liberated from strangulation, which can be replaced in a moment within the belly, should leave it in the hernial sac. A more extended experience of herniotomy would show Dr. Girard that such cases are not so very uncommon; and whatever course may be thought best in cases of injured bowel or of excessively large protrusion, no other course than complete reduction can ever, we are confident, be generally adopted in ordinary cases of hernia. The second objection to leaving the gut in the sac, is the difficulty of being certain whether the strangulation has really been overcome. This objection Dr. Girard does not quite pass over, but his mention of it on p. 232 is far from satisfactory, and shows more of the student than the operator. No surgeon would feel satisfied in leaving a portion of the gut in the sac, unless he had passed his finger freely into the peritoneal cavity; and if this had been done, what becomes of the strict separation between the general cavity and the hernial sac? Thirdly, nothing, as far as we can see, can prevent the recurrence of strangulation. Dr. Girard has stated (p. 234) that no return of strangulation has occurred in the cases which have presented themselves to his reading; but such return of strangulation does occur (though rarely) from reprotrusion after the complete reduction of the herniated bowel, and surely the event would be more probable if the gut was left filling up the neck of the sac. For all which reasons we are far from being convinced by Dr. Girard's work, and shall continue the old practice of entire reduction of the bowel whenever this can be effected without obvious danger of doing mechanical injury to the gut.

We have thought Dr. Girard's book worthy of a somewhat prolonged notice, because it is really very ably and well written; and if we are correct in believing that Dr. Girard is still little more than a student, it is the work, at any rate, of a student of no ordinary promise. Considered in itself, too, and as to its bearing on practical surgery, the many proofs which this work has accumulated of the advantage of leaving the bowel unreduced in certain cases (the nature of which we have indicated above), ought not to be without their influence on the treatment of such very embarrassing cases. We are sure that we have frequently seen lives lost by too great anxiety on the part of the operator to push the gut into the belly. Dr. Girard will have done the world good service if he can succeed in eradicating this prejudice.

PART THIRD.

Original Communications.

Art. I.

Case of Vesico-Intestinal Fistula for which Colotomy was performed, with Remarks. By Thomas Bryant, Assistant-Surgeon to Guy's Hospital.

In March, 1867, I was requested by my colleague, Dr. Habershon, to see William D—, a patient of his in Guy's Hospital, who was suffering from supposed stricture of the rectum with bladder symptoms, when the following history of the case was obtained.

The man was a cellarman forty-nine years of age, married, and the father of ten children. When twenty-one years of age he went to the West Indies, where he had yellow fever and ague: he returned home after five years in good health, and remained well for twenty-one years, or till two years ago, when he was seized with a violent attack of purging and the passage of a quantity of blood; from that time he has passed at uncertain intervals slime and blood in small quantities with his motions.

Eight or nine months since he passed some ounces of blood from the bowel, and seven months since he began to have pain across the hypogastrium and about the anus. Six weeks after noticing this pain he observed flatus and a small quantity of blood and fæces pass with his urine—of late he has suffered at times from difficulty in defecation and frequent desire, but the act has not been associated with pain. His motions have, however, never been well formed since his illness.

For the last few months his general health has suffered much, and he has lost both flesh and strength. He has never had syphilis. He had cough.

His chest was carefully examined by Dr. Habershon, who found the resonance at the apex of the left lung to be imperfect, and the respiration coarse and bronchial; these symptoms subsequently became more marked: His abdomen was not distended, but on the contrary was tolerably soft and supple; pressure however in the hypogastric region gave pain. No tumour or abdominal swelling could be detected.

The urine was loaded with feculent deposit, and at times solid fæces passed per urethram. The act of micturition was always painful, and at times the pain was very severe. On making a careful examination of the rectum, the whole calibre of the lower bowel, as far as the finger could reach, was superficially ulcerated, but this ulceration was not associated with much adventitious deposit, nor with any such as usually exists in cancer; the bowel appeared to be fixed in its position, and glued to the parts around by inflammatory lymph: a stricture of the bowel was clearly felt high up, through which the tip of the finger could just be passed. Pressure forwards towards the neck of the bladder gave great pain, and the whole of the examination caused severe distress.

The nature of the case was thus tolerably clear. It was one of ulceration and stricture of the lower bowel, with a fistulous communication between the bowel and the bladder. It was fair to believe that this communication was indirect, from the fact that no urine made its way into the bowel; and it appeared probable that the large intestine was the part involved, from the solid nature of the fæces passed per urethram. As to the nature of the disease it should be stated, that there was no distinct evidence of the case being of a cancerous nature, and that the probabilities tended towards an opposite conclusion.

With the facts of the case as just related before me, there remained but one line of practice open by which relief to the patient's sufferings could possibly be afforded; for it was certain, that as long as fæces passed down the bowel to the seat of mischief, and were obstructed at their natural outlet, they would find an exit through the fistulous opening into the bladder, and that all the miseries of a foreign body in that organ would permanently remain to be added to those caused by an obstruction to the bowel.

Under such circumstances colotomy was suggested; and with Dr. Habershon's sanction, and the patient's permission, I performed the operation on April 27th, the objects I had in view being to make a free outlet for the intestinal contents above the seat of the disease, and thus prevent the passage of fæces through the fistulous opening into the urinary passage, with the hope that the opening into the bladder might subsequently contract, if not close, and the ulceration of the bowel also heal,—the tolerable certainty existing that relief to the patient's suffering would be afforded for a time, if not permanently secured.

The operation.—The abdomen of the patient was not much distended, nor the left loin prominent; as a consequence it was expected that some difficulty would be experienced in finding the colon: it became a point of importance, therefore, that plenty of room should be given by the incision through the abdominal walls to allow of manipulation, and with that view I made an oblique incision about four or five inches in length, beginning one and a half inches to the left of the spine below the last left rib, and passing downwards and forwards in front of the anterior spine of the crest of the ilium. The line of incision passed obliquely across the external border of the quadratus lumborum muscle about its centre, and took the same direction as the nerves which traverse this part.

The operation was performed without difficulty, and two small vessels only had to be tied. The kidney came into view at once after the muscles had been divided, and was pushed up. The bowel was completely collapsed and out of view; it was hooked up, however, without difficulty by the finger and thumb, and secured firmly to the integument by four sutures; it was then opened, and solid fæces soon passed. Within one hour of the operation fæces flowed in abundance.

The patient was much relieved by the operation, and in two days the urine became clear and natural, no fæces passing per urethram.

On May 5th, or the ninth day after the operation, the patient complained of some little pain in the perinæum, and on examination a slight fulness was detected in that part. This symptom was also accompanied with deep-seated pelvic pain and evidence of emphysema in the pelvis, as indicated by crepitation on the pressure of the hand over the region of the bladder.

On May 10th, all these symptoms had become more marked, and on examining the perinæum and pelvic organs from the rectum, it was clear that an abscess existed at the base of the bladder. A free incision was accordingly made in the perinæum in the middle line, laying open the urethra, and a quantity of horribly fetid fluid composed of pus, urine, and fæces, made its escape. The patient experienced much relief by this operation, and for some weeks afterwards everything went on well. The wound in the loin rapidly healed, and firm union between the bowel and integument took place; fæces flowed without difficulty through the artificial anus, and a small quantity passed at times through the natural channel. The urine remained clear and free from fæces, passed without pain or even inconvenience, and all bladder symptoms disappeared; the perineal wound also rapidly closed.

The man's health much improved, and food was taken freely with appetite. On June 20th, however, nearly eight weeks after colotomy had been performed, a change took place. Abdominal pain again made its appearance in the region of the bladder, which at times became very severe. The coils of small intestine below the umbilicus became much distended, and their outlines could be readily made out through the thin abdominal parietes. Pressure over these parts caused great pain; constipation was present, and some constitutional disturbance. Fomentations over the hypogastric region, and saline purgatives with enemata, gave partial relief, but the symptoms remained, varying much in their severity till June 29th, when the man suddenly passed a quantity of liquid fæces and flatus per urethram. With this symptom all others disappeared. Pain subsided; the distension of the bowels vanished and relief was experienced.

From this date, however, his general health began rapidly to fail. The urine was always loaded with thin liquid fæces, evidently from the small intestine; it flowed with tolerable freedom, and no such difficulty was ever experienced in its passage as had existed in the earlier period of the disease. Solid fæces passed daily through the artificial anus, but in diminished quantity. Small portions were likewise evacuated through the natural channel; but it was observed that the motions which came by the natural passage were generally liquid, when those by the artificial were solid.

The man gradually sank from exhaustion, and died on August 27th, four months after the operation of colotomy, and two after the reappearance of fæces in the urine.

The *post-mortem examination* was made by Dr. Pye Smith nineteen hours after death, when the following report was drawn up: The opening where the operation of colotomy was performed appeared quite healthy, the bowel and integument having thoroughly united.

In both lungs there existed groups of miliary tubercles, some coalescing. In the left lung tuberculosis had advanced beyond that in the right, there being a large vomica at the posterior part of the upper lobe.

The heart was natural and healthy as were the large vessels.

The stomach and small intestines were inflated with gas. There was no evidence of recent or general peritonitis, and no ascites.

The small intestines contained a small quantity of thin, light, coloured fæcal matter. Rather more than a foot from its termination a coil descended into the pelvis and had become firmly attached to the bladder; coming up again from the pelvis of rather less calibre, it entered the cæcum in the usual place.

The cæcum was healthy; the colon was much contracted and opened into the wound in the side, to which it was firmly united. Its calibre was uninterrupted throughout; it contained a little thick brownish fæces. All the adhesions about the wound were very firm and quite healthy.

The rectum descended normally from the sigmoïd flexure, but in the pelvis became adherent to the bladder, and opened into a large abscess. The calibre of the rectum, however, continued open to the anus.

All signs of ulceration had disappeared, and with the exception of the large opening into the cavity at the base of the bladder no signs of disease in the rectum remained.

This abscess at the base of the bladder communicated also with the small intestine and bladder.

Right kidney was large, anæmic, but otherwise healthy. *Left* small, pelvis dilated into a large abscess with thick walls, and contained thick greenish pus and cretaceous matter; the left ureter was also dilated and full of pus.

Bladder very small and filled with thick greenish pus. Between it and the rectum was a large abscess with dense adhesions all around it. Into this opened first the ileum where adherent to the bladder as above described; next the rectum by a large orifice; and thirdly, the bladder itself, by a smaller one just enough to admit the finger.

Remarks.—In reviewing the history of this case with the light thrown upon it by the necroscopic examination, there is no difficulty in recognising its true nature, nor in understanding the process by which the changes found to exist were brought about.

There is little room to doubt that the case was one from the beginning of simple ulceration of the lower bowel; for at the end no signs of ulcerative disease existed beyond that furnished by the fistulous vesical communication; and there is no reason to believe that this ulceration of the rectum had anything to do with a dysenteric affection.

That this ulceration of the lower bowel made its way into the bladder was clearly proved by the passage of flatus and fæces with the urine per urethram, one year and a half after the first appearance of the disease; and that the communication was indirect or valvular, was indicated by the fact that no urine was ever observed to have made its way into the rectum.

The benefit afforded to the patient by the operation of colotomy was immediate and striking, although its duration was for a time interfered with by the formation of the fæcal abscess at the base of the bladder; and the relief which was given by a free incision through the perinæum into the abscess was rapid and effectual.

The secondary communication of the small intestine with the bladder by ulceration, two months after colotomy had been performed, was a point of special interest, although the full benefit of the operation was fairly marred by such an incident.

And lastly, had not suppuration of the kidney existed, there is good reason to believe that life might have been indefinitely prolonged.

The case with its details as just recorded will be read with interest by all who have studied the paper published by Mr. Holmes in the last volumes of the 'Medico-Chirurgical Transactions,' for it tends to support the conclusions which were there deduced from the considerations of his own and other cases of a somewhat similar nature.

It supports the opinion "That ulcerated openings sometimes take place between the bladder and either the large or small intestines, many of which have no connection with previous stricture of the gut, still less with cancer:" for in the case I have related the ulceration was evidently of a simple kind; it occurred in the rectum as well as in the small intestine, and had apparently no connection with cancer, syphilis, or even dysentery. Indeed I would venture to suggest that these cases appear to be somewhat analogous to the simple perforating ulcers which are found in the stomach and duodenum, and with which physicians and pathologists are quite familiar. They are of the same nature as the simple perforating ulcers which make their way into the peritoneal cavity or ovarian cyst, the bladder in these cases being the nearest cavity into which the ulcer by its situation can possibly open.

It supports the conclusion "That in those cases in which the fæces discharged into the bladder come from the lower gut, and are consequently more solid, great suffering is produced:" for the difference in the symptoms in the case I have related at the early and late period of the disease was very great. At the beginning, when solid or large intestine fæces passed into the bladder and per urethram, the agony at times was most severe. At the end of the case when the liquid or small intestine fæces made their way into the same channel and were passed with the urine, the pain, although troublesome, was not to be compared with what the patient had previously suffered.

And, lastly, it tends to prove "That in such of these cases as are not dependent on malignant disease, colotomy, by diverting the fæces from the fistulous channel, may enable the latter to close, and thus relieve the patient's sufferings, and restore him to a condition of health and of comparative comfort:" for it is an indisputable fact that after the operation of colotomy had been performed, the ulceration of the bowel gradually healed, and that the only evidence of its previous existence which remained was the fistulous vesical communication. There is good reason to believe that this opening might have contracted subsequently and even closed, had not other complications appeared to shorten life. To procure this end, it should be added that I expressed a wish to the patient after the contents of the small intestine had found their way into the urinary passages to lay the bladder and rectum into one as in the recto-vesical operation for stone, trusting that by such means the fistulous openings with the line of incision would subsequently contract as in the lithotomy operation.

In conclusion, I would say one word in favour of the oblique line of incision which was adopted in the case just related. It appears to offer advantages over the transverse of Amussat or the vertical of other surgeons, in giving more room for manipulation in cases in which there is a strong probability that the colon is empty, and in which it may have to be looked for towards the pelvic brim; it takes the line of the nerves and vessels which pass forwards from the lumbar region, and, as a consequence, diminishes the prospect of any unnecessary injury being sustained by any of these parts. It follows the ordinary integumental fold of a patient when assuming the recumbent posture and thus favours repair; whilst it possesses in addition all the advantages which are sought by any other line of incision.

NOTE.—I have performed colotomy in three instances since the above case was recorded, and have found the oblique line of incision most advantageous.

ART. II.

On the Prevention of Contagious Venereal Disease. By JAMES R. LANE, F.R.C.S., Surgeon to, and Lecturer on Surgery at, St. Mary's Hospital; Senior Surgeon to the London Lock Hospital, &c.

THE question of the prevention of contagious venereal disease by legislative interference has of late been attracting a great deal of attention, not only in the medical profession but in the minds of the public at large. The subject is one the importance of which can hardly be exaggerated; and, when we consider the prevalence of this form of disease amongst all classes of the population, the various evils, immediate and remote, of which it is the cause, its hereditary character, the many ways in which the innocent as well as the guilty may become its victims, and the generally pernicious influence which it exercises over the health of the community, I do not think it is going too far to speak of it as one of the most urgent of the sanitary questions of the day.

It is on sanitary grounds alone that I am desirous of directing further attention to it, for with respect to prostitution I fear we must be content, in our present state of civilisation, at all events in all populous communities, to accept it as a disagreeable but inevitable necessity—an evil which it would be hopeless to attempt to suppress. Many, I dare say, would think that, even if it were possible, it would not be desirable to do so; that we had better bear the ills we have than fly to others that we know not of. But however that may be, I would especially insist that, in considering the propriety of restrictive measures, we need not concern ourselves with the question of prostitution at all. Such measures need not affect in any way either its suppression or its toleration, and need not involve any such public recognition of prostitutes and brothels, as is the case in most continental towns. The main point at issue is simply the sanitary one, whether we can prevent, or at all events diminish, the alarming amount of venereal disease now prevalent amongst us.

That this is by no means a hopeless task may, I think, be abundantly shown by the results already obtained on a limited scale, and in certain limited areas, by means now in operation. These means no doubt entail police supervision, and to that extent recognition, of women known to be prostitutes, but this need be attended with no *publicity* whatever; and so far from being an encouragement to immorality, as some have feared might be the case, or offering inducements to others to enter upon such a mode of life, the effect is decidedly deterrent, while the influence exerted over those already belonging to this unfortunate class can be clearly shown to be beneficial both in a moral and material point of view. It has been made the means of rescuing many from a condition of degradation, and of placing those who are really desirous to reform (and there are many who are only too glad to have the opportunity) in a position to obtain their livelihood by some honest employment.

I shall, I think, best promote the object I have in view if I give a brief history of what has lately been done, and is now doing, in this direction. The frequency of venereal disease in the army and navy, and the enormous cost entailed on the country by the invaliding of soldiers and sailors from this cause, became at last so seriously felt, that in 1864 an Act was passed, the intention of which was to provide for the treatment and seclusion of diseased prostitutes in some of the military and naval stations. It proved, however, ineffectual, because it contained no provision for the periodical examination of women, and did not give sufficient power to the authorities to erect and maintain hospitals in which they could be treated. Consequently, in 1865, a medical commission, under the presidency of Mr. Skey, was appointed by the Admiralty to inquire into and report upon the whole question. In accordance with the report of this commission, an amended Act was founded, which passed through Parliament in 1866, and came into operation on the 1st October in that year.

This Act specifies certain districts—military and naval stations, such as Portsmouth, Plymouth and Devonport, Aldershot, Woolwich, Chatham, Sheerness and others—within the limits of which every woman known to be obtaining her living by prostitution is placed under police supervision, and when required to do so by the police, must present herself for examination by a medical officer appointed for the purpose, and must attend before him for periodical inspection at whatever intervals he may prescribe. If found to be suffering from any contagious venereal disease, she is sent by his order to one of the hospitals provided under the Act, and detained there till cured. While in hospital she is in the legal custody of the governing authorities of the institution, and cannot leave till she is discharged by the medical officer, with a certificate that she is free from contagious disease. For any infraction of these rules, or for any misconduct while in hospital, she is liable to be taken at once before a magistrate and sentenced to a term of imprisonment. Under the Act it became necessary for the government to provide sufficient accommodation for the treatment of the diseased women. Accordingly at some of the stations, hospitals have been erected by the War Office or the Admiralty, while at others it has been found more convenient to hire beds at hospitals already existing. The former plan has been adopted at Portsmouth, Aldershot, Shorncliffe, and Colchester; the latter at Devonport, Sheerness, Chatham, Woolwich, and Windsor. In the London Lock Hospital 120 beds are now allotted to patients from Woolwich, Chatham, and Windsor, and also from Aldershot when the hospital there does not afford sufficient accommodation.

The essential principles of the Act, then, are first, compulsory periodical medical examination of all women known to be prostitutes; and second, the provision of adequate hospital accommodation for those found to be diseased, and of course compulsory detention until cured.

The working of this measure has been entrusted to the Metropolitan Police. A superintendent of police, Mr. Mallalieu, has been appointed Inspecting Officer for all the stations, and to him the entire management has been confided. Under his orders the requisite number of police constables have been dispatched to each station, great care having been taken to select men of intelligence, temper, and good moral character: and they have been specially enjoined to treat the women with kindness and forbearance. The way in which they have performed this difficult and delicate duty is deserving of the highest commendation, as I can testify from my own personal observation, which is fully confirmed by the evidence from various independent quarters given before the Committee of the House of Lords last session.[1] It is there stated that, for a number of 5479 women passing through the hands of the police, it was only necessary to employ the penal clauses of the Act in six cases. Under this judicious management very little difficulty has been experienced in inducing the women to submit to examination. They have been brought to look upon the police and the medical officers as their friends; and they are, for the most part, not only willing but glad to conform to the provisions of the Act, most of them having sense enough to see how greatly it operates to their benefit. The irksomeness of detention in hospital for what they believe to be trifling causes leads to occasional slight outbreaks, as might be expected where a number of women of indifferent character, and unaccustomed to control, are collected together; but, on the whole, it is surprising how little trouble of this kind has been met with.

The system has now been in operation for two years; but some time, of course, elapsed before it could be brought into proper working order, and before the requisite number of beds could be provided. Indeed, this has even yet been hardly accomplished at all the stations; consequently, the periodical inspection has been necessarily irregular and insufficient, and is only now beginning to be carried out in a thoroughly effectual manner. Nevertheless, whenever the plan has had anything like a fair chance, the result has been in the highest degree satisfactory. Everywhere the improvement in the health and general condition of the women has been remarkable. The diminution of disease in the troops has also been great, but this has varied in degree at the different stations from causes which admit of easy explanation. The success obtained has been everywhere proportioned to the completeness with which the provisions of the Act have been enforced; thus, at Sheerness, which is an isolated station, and where there is but little intercommunication with the surrounding district or with any neighbouring town, either on the part of the women or of the men stationed there, the disease has been almost obliterated, although Sheerness was in former years one of the worst places in the kingdom in this respect. From evidence given before the Committee of the House of Lords, it appears that, in August, 1867, there was not a single case of disease in the men at this station; and during the three months ending June 30, 1868, the amount of disease in the garrison was only from ·46 to ·72 per cent. per month. During the same three months only 16 women were found to be diseased. This is a very striking result; it is owing partly, no doubt, to the way in which the place is isolated; but still more to the strict manner in which the provisions of the Act have been enforced. Sheerness is, in fact, the only place where there has been always adequate hospital accommodation, and in which it has consequently been possible to carry out a regular periodical examination. The regular inspection, not only of those suspected to be diseased, but of all public women, healthy and diseased alike, is the *sine quâ non* of success in carrying out a measure of this kind.

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[1] See 'Report of the Select Committee of the House of Lords,' on the 'Contagious Diseases Act,' July, 1868.

Next to Sheerness, the most satisfactory results have been obtained at Plymouth and Devonport, where there has been latterly ample hospital accommodation, and consequently periodical examination has now begun to be properly enforced. The improvement at this station may be shown by the fact that, in April, 1865, out of 8583 soldiers and sailors, 180, or 2 per cent., were admitted to the hospital, while, in April, 1868, out of 10,635 men, only 69, or ·64 per cent., required admission.[1] The monthly per-centage of men diseased, during the quarter ending June 30th, varied from ·64 to 1·07 per cent., which is not greatly in excess of Sheerness. A good illustration of the beneficial effect obtained is afforded by the returns from the Royal Marine Division at Plymouth. For the first six months of 1864, before the passing of either Act of Parliament, the number of diseased men per 1000 was 129·7. This proportion has gradually and steadily diminished, till, for the first six months of 1867, it was only 49·3 per 1000.

At Portsmouth the proportion has been somewhat higher than at Plymouth, and has varied from 1¼ to 2 per cent. per month during the same period; but at Portsmouth there has been until recently, if there is not still, an insufficiency of hospital accommodation.

At Woolwich and Chatham the result, as regards the troops, has been less marked than at Sheerness and Plymouth, in consequence, no doubt, of the necessary conditions to which I have alluded, having been all more or less wanting. The Woolwich district, for instance, only comprises the parishes of Woolwich, Plumstead, and Charlton, so that there are Deptford, Greenwich, and London within easy distance on one side, and Gravesend on the other, where the women are uninspected, and to which the soldiers have free access; while it is equally impossible to prevent women from these places coming into the protected district and communicating disease to the men. Many diseased women, also, are found to come from a distance into the inspected districts for the purpose of getting sent to hospital for treatment.

Chatham is equally unfortunate in having Gravesend on one side and Maidstone on the other, both of which are unprotected. Periodical inspection also has never, till quite recently, been attempted in these places; the examinations have been confined to those suspected of being diseased; and as these have furnished sufficient cases to fill the beds which were provided, it was obviously useless to carry the inspection further.

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[1] See 'Report of the Select Committee of the House of Lords,' 1868, p. 4.

Another disturbing cause has been found in the fact that the troops in garrison towns are continually changing, and therefore regiments from unprotected districts are constantly coming in and bringing disease with them, which they communicate to the women on the care of whom so much trouble has been bestowed. So much has this been found to be the case at Aldershot, that, at the request of the Secretary of State for War, an order has been issued that all regiments coming into camp should be inspected by their medical officers, and all men found to be diseased at once sent into hospital. This very obvious precaution had been omitted on account of the reluctance of regimental surgeons to make the necessary inspection. The regulation is also very properly applied to men returning from furlough, who have been found to be a fertile source of disease. Notwithstanding these drawbacks, the improvement at this station has been very decided. In 1865 the annual rate of admission into hospital per cent. on the strength of the troops was 30·2, while by the police returns for the September quarter of 1868 it was 21·4, and for the June quarter 18·7 only.

At Aldershot the Act is being very efficiently carried out under the management of Dr. Barr, late House Surgeon at the London Lock Hospital, who is now in charge of the Female Hospital there, and Medical Inspector of Women for the district. He is enforcing to the utmost of his power the periodical inspection, but finds a serious disturbing cause in the influx of fresh women who follow the different regiments and bring disease with them. The number of prostitutes at Aldershot is about 300; the majority are permanent residents in the neighbourhood of the camp, but a very considerable proportion are migratory, from the cause above mentioned. For these 300 women 70 beds are provided—50 on the spot and 20 at the London Lock Hospital. The average number always under treatment is about 60, or one fifth of the whole number. Dr. Barr has been good enough to send me a copy of his return for the quarter ending Sept. 30th, 1868. During this quarter the number of attendances for inspection was 871: whereas, in the previous quarter, it was only 626, showing an increased efficiency in this respect of 245 attendances. The number sent to hospital was 228; that is to say, a population of 300 women furnished, at Aldershot, in three months, 228 cases of disease; in other words, they become diseased at the rate of something more than 300 per cent. per annum. The amount of disease would, of course, not be nearly so great were not so many of them only temporary residents; but it is easy to imagine how much mischief is prevented by their early seclusion.

At Aldershot among the women, not more than one third were cases of syphilis, the rest were gonorrhœa; and it is worthy of notice that three fourths of the syphilitic cases were furnished by new comers, and only one fourth by the old residents. The enforced early treatment greatly facilitates their cure, and shortens the residence in hospital, thus materially diminishing the expense. The average time at Aldershot during this quarter was 22¼ days. It is interesting to contrast this with the time required for the *voluntary* patients at the London Lock Hospital, which in 1867 averaged 50 days. The average of diseased *men* at Aldershot is about 1¼ per cent. This (though considerably in excess of Sheerness and Plymouth, where the highest figure is hardly more than 1 per cent. *per month*) may, I think, under all the circumstances, be looked upon as a very favorable result.

During the quarter ending September 30th, 1868, taking all the inspected stations together, out of a total force of 55,534 soldiers, sailors, and marines, 3221 men were admitted into hospital for venereal disease, but it is worthy of remark that only 1873 contracted their disease within the inspected districts; that is, to-day, as much as 42 per cent. of the cases were caused by women not under the operation of the Act. For this force of 55,534, the average number of men in hospital during the quarter was 829, or 1·5 per cent.[1]

The general beneficial result obtained may be thus expressed. In 1864, the annual ratio of admissions to hospital for venereal disease throughout the whole army was 29 per cent. This has been reduced at the inspected stations, taking them collectively, to 23·2 per cent. But it must be remembered that nearly half the disease now existing is contracted out of the inspected localities. The annual ratio of men contracting disease in the districts where they are stationed was not more than 13·5 per cent. in the September and 10·4 per cent. in the June quarter, 1868. These figures, however, only state the *numerical* diminution, they give no expression to the diminished virulence of the disease which has been everywhere so remarkable.

The following facts relating to the women are taken from the police returns for the quarter ending September 30th, 1868. The Act is now in operation at eight stations, viz., Aldershot, Chatham, Devonport, Portsmouth, Sheerness, Shorncliffe, Windsor, and Woolwich. Colchester will be included as soon as the hospital there is completed. The total number of women brought under its provisions during the quarter was 3432. Of these, 1296 or 36·6 per cent. were found diseased and sent to hospital; 2011 were free from disease; 225 were for various reasons not examined. On their discharge from hospital, 1110 have returned to their former pursuits, 48 have entered reformatories, and 79 have returned to their friends. Only one required to be proceeded against for refusing to submit to examination. The number of beds provided for their reception is now 517. This return shows a decidedly increased efficiency over the previous quarter, in which the number of women under supervision was only 2542 and the number treated 1094. The number of beds has also been increased from 484 to 517 since June 30.

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[1] This return is much less favorable than that for the June quarter, showing an average in hospital of 1·5 as compared with 1·1 per cent. The increase is probably due to the more frequent movement of troops from unprotected into the protected stations during the latter period.

With reference to the beneficial effect of restrictive measures on the women themselves, I can speak with confidence from my own personal observation. There are at present in the London Female Lock Hospital 150 beds. Of these, 120 are occupied by prostitutes sent under the Contagious Diseases Act from Woolwich, Chatham, Windsor, and Aldershot. The remaining 30 are filled by patients applying voluntarily in the ordinary way. The hospital, therefore, affords a good opportunity of comparing the condition of women under the voluntary and compulsory systems. The contrast between them is very strongly marked, both as to the character of disease and the severity of the symptoms. Among the ordinary or voluntary patients are found primary ulcerations of a severe form, often of a phagedænic or sloughing character, and frequently accompanied by suppurating buboes; severe secondary symptoms, with ulcerating mucous tubercles on the genitals in an aggravated form; tertiary ulcerations and affections of the bones. Diseases of the above character which may be classed collectively under the head of syphilis, constituted in 1867, 80 per cent. of the admissions, and in 1868, to September 30th, 78·75 per cent. The remainder, which may be grouped together under the term gonorrhœa, consisted of vaginal and uterine discharges, usually attended with some complication, such as warts, severe excoriations, labial abscess, or inflammatory bubo.

In the Government or compulsory patients, on the other hand, the "syphilitic" cases consist for the most part of primary sores, usually of small size, very rarely phagedænic, rarely attended with suppurating buboes, and readily yielding to treatment. Secondary symptoms are rare, and usually mild in character; the extensively ulcerated mucous tubercles, so common in the voluntary patients, are almost unknown. Further, the "syphilitic" cases in the government wards constituted in 1867, only 41 per cent. of the admissions, a proportion which has been still further reduced in the first nine months of 1868 to 34·8. The remaining, or "gonorrhœal" cases consisted almost exclusively of simple uterine and vaginal discharges, rarely complicated, as in the voluntary patients with either excoriations, warts, or inflammatory buboes.

The difference in the time required for the cure of the two classes of patients is also remarkable. Thus, in 1867, the stay in hospital of the ordinary (or voluntary) patients averaged 50 days; while that of the Government (or compulsory) cases only averaged 31 days. In 1868, up to September 30th, the average of the former has been 39·3 days, of the latter 28·15 days. These figures, however, only partially express the real difference between them, because the voluntary patients can, and often do, leave by their own wish, when their more severe symptoms have abated, while the compulsory cases must remain until they can be certified as absolutely free from contagious disease. The figures also give no idea of the real difference in the general sanitary condition and appearance of the two classes of women.

The system of inspection then leads to a marked diminution in the severity of all these affections, and produces a much larger proportion of the milder or gonorrhœal, as compared with the more severe or syphilitic class. The progressive improvement, shown by the per-centage of 1868, as compared with 1867, both in this respect and in the time required for treatment, is also a very significant and encouraging fact; for when these patients were first received in 1864, and up to the time when the present act came into force in October, 1866, they presented types of disease quite as severe, or even more so, than we had ever been accustomed to admit in the ordinary way.

It should be mentioned that readmissions under this system are frequent. One woman has been in the London Lock Hospital twelve times since Oct. 1866, a considerable number have been inmates three, four, five, and six times. In fact, during the first nine months of 1868, 853 admissions only represent 629 women. Many of these readmissions are in consequence of distinctly fresh contagion, but the majority are cases of uterine and vaginal discharge, which are readily cured by a two or three weeks' residence in hospital, but which speedily recur when the woman returns to habits of prostitution. Purulent discharges from the cervix uteri, with abrasions or ulcerations of the os uteri, are exceedingly common in prostitutes; they are probably the result, not of any specific contagion, but of the constant irritation and excitement to which their generative organs are subjected. There can be no doubt, however, that they are a fertile source of gonorrhœa and its complications to the men with whom they associate.

It is a very striking and a very gratifying fact, that not only has the *quantity* of disease been diminished in the localities where the Contagious Diseases Act has been in operation, but its *quality* has been modified in the most marked manner. Not only has the proportion of syphilitic disease to milder affections been much lessened, but the syphilis which has occurred has been of a mitigated character, and secondary affections have been rare. The testimony to this effect from all quarters, both as regards the women and the men, is remarkably uniform.

The necessity for careful and continuous supervision becomes evident when we consider how large a proportion of these women become infected. According to the police returns; in the September quarter, 36·6 per cent. or more than 1 in 3; and in the June quarter 43 per cent., or more than two in five, were admitted into hospital. The amount of disease which may be prevented by their timely seclusion must be very great; for it is probably not too much to say that each of these women, for each day she was allowed to be at large, would communicate disease to at least one soldier.

There is reason to believe that there has also been a very considerable diminution of disease, in the civil population of the inspected localities, though accurate information on this point is, of course, very difficult to be obtained. Dr. Stuart, the Medical Inspector for Woolwich and Chatham, who has made inquiries, tells me that at Woolwich the very great majority of venereal cases occurring amongst the working classes are treated by the druggists, and especially by one or two who advertise for practice of this kind. When questioned, they all agreed as to the greatly diminished amount and mitigated severity of the cases coming under their notice. One who carried on a very thriving trade of this kind stated that he now does very little as compared with former years, and that the cases are milder and more readily curable. Another said that whereas he used formerly always to keep daily a large bottle of Mistura copaibæ ready prepared, he had lately discontinued it altogether, finding it so little in demand.

As regards the women, there is a moral as well as a medical and sanitary side of this question, on which I will add just one word. There can be no doubt that their supervision by the police, their coming in contact while in hospital with medical officers and nurses, their subjection to discipline and to enforced habits of cleanliness and order, combined with the religious instructions of the chaplain, and the generally kind and considerate treatment they have met with, have exercised a very humanising influence over them. They have been led to reflect and to look a little more in advance of the present moment, to respect themselves more by finding that there were people who were ready to care for them and take an interest in their welfare, and their demeanour and habits have become less abandoned and reckless in consequence. Opportunities have always been afforded them of leaving their mode of life, if they wished, by entering homes and reformatories, and of these a considerable proportion continually avail themselves. During the September quarter 3·7 per cent. of all the women treated under the Act entered asylums, and 6 per cent. are stated to have returned to their friends. Of those admitted into the Lock Hospital, 34, or about 5 per cent., were admitted into the Lock Asylum in 1867, and the proportion has been about the same during the present year.

The foregoing is a brief summary of what has been effected during the last two years by police supervision and compulsory medical treatment of diseased prostitutes, with a view mainly to the prevention of venereal disease in the military and naval forces of the country. The shortcomings of the present measure can be shown to depend almost entirely upon the too limited area of its operation; and all the evidence points to the expediency of extending it as widely as possible without loss of time, and for the benefit of the civil as well as the military population. During these two years the subject has been attracting more and more of public attention. A committee of the Harveian Society collected a large mass of evidence, showing the great prevalence of this form of disease, and published a report urging the necessity for some general restrictive measures for its prevention. The proceedings of the Harveian Society led to the formation of an association for the purpose of extending the benefits of the present Act to the general population. This association now numbers 450 members, amongst whom are 32 members of parliament and 53 clergymen. Amongst the latter are found the names of bishops, deans, and archdeacons, together with many eminent for their position in connexion with education, such as the Vice-Chancellors of Oxford and Cambridge; the Masters of Trinity and of Jesus Coll., the Regius Professor of Divinity, Cambridge; the Warden of Winchester; the Principal of King's College, London, and others. During the last Session of Parliament, a Select Committee of the House of Lords, moved for by Lord Lifford, has been sitting to consider the operation of the present Act and the propriety of extending it, and has collected a large amount of important and most conclusive evidence in favour of so doing. To the report of this Committee, and also to the reports of the association and of the Harveian Society, I would refer those who are desirous of further information on this matter.

Under this powerful, enlightened, and rapidly increasing influence, there is reason to hope that some further legislative action will shortly be undertaken to check this most serious evil. The Select Committee of the House of Lords have recommended a Bill giving power to the Queen in Council to apply the Act of 1866, first, to *all* military and naval stations, and secondly, to any locality the inhabitants of which may apply to be included in its operations, and may be able to show that adequate hospital accommodation can be provided and maintained, and proper arrangements made for the religious and moral care of the inmates.

I am myself most desirous of seeing a measure which shall be universal in its operation; but I think, nevertheless, the proposition of the committee may be judicious; it has, at all events, the merit of being safe, and it would at once secure a large instalment of what is required. Many large towns have already formed branches in connection with the association to which I have alluded, and are desirous of being included in the operations of the present Act; and their example would, no doubt, soon be largely followed. It would be highly desirable that the principal seaport towns should be brought under the system as soon as possible, for these are the head-quarters of venereal disease. Its prevalence amongst our sailors has obtained for them a most unenviable notoriety in foreign ports, where they are regarded by the authorities as a fertile source of disease, and as being one of the principal obstacles to the efficiency of their preventive measures.

The metropolis ought certainly to be included at the earliest practicable period. An Act of this kind could be enforced in London, through the agency of the Metropolitan Police, with probably greater facility and greater efficiency than in any other place.[1] There are in London 5,628 prostitutes of the lowest order, classed with thieves and other bad characters,[2] whose habitations are known to the police, and who could be readily reached at any time for inspection. In the east end of London, and in such places as the low parts of Westminster, the amount of venereal disease which might in this way be withdrawn from circulation would be enormous.

There is but one serious difficulty, and that is the expense of providing the necessary hospital accommodation, and the question as to how this expense should be met, whether by local taxation or by the general funds of the state, or partly by the one and partly by the other, which latter would probably be the more equitable plan. I am not sufficiently versed in political economy to pretend to solve the question of the proper incidence of taxation in such a case, but I am strongly of opinion that what other countries can afford to do, and to do effectually, it would be a disgrace for England to shrink from on the sole ground of expense. The moment appears to me to be a very favorable one for the agitation of this question, in London especially, where large pauper infirmaries are in course of erection, in which arrangements might be readily made at first starting for the establishment of separate wards for this purpose. The number of beds required, and consequently the expense, would at first be considerable, but fortunately it would gradually diminish as the disease was brought under control. Such an expenditure would in the end prove the truest economy by diminishing one of the most fertile sources of disease and debility amongst our population.

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[1] See evidence of Capt. Harris, Assistant Police Commissioner; Report of 'Lord's Committee,' p. 83.

[2] See 'Blue Book,' entitled 'Judicial Statistics.' 1868, p. 9.

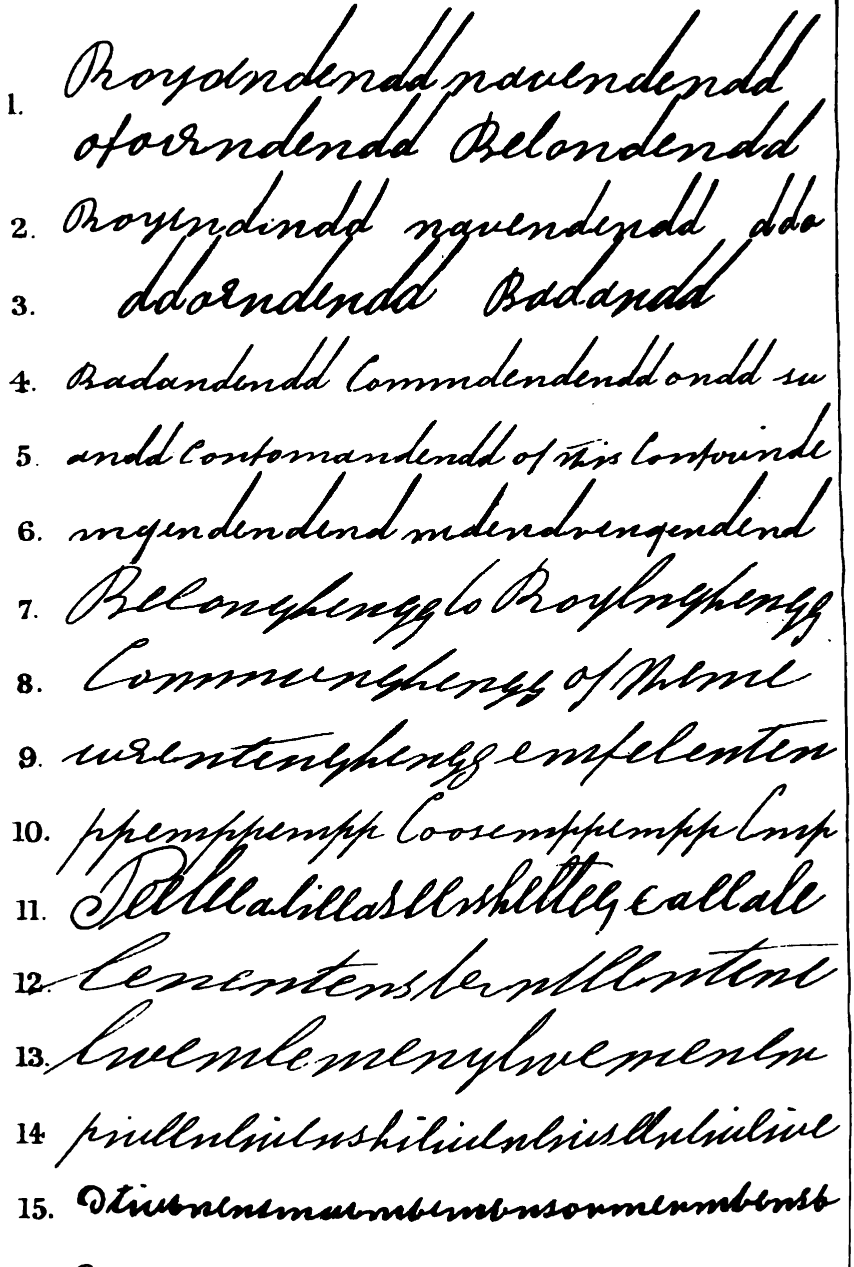
One ready mode of providing hospital accommodation without expense to the country might be found for all seaport towns, and also for London, by the appropriation of a few of the obsolete and useless wooden vessels which now encumber the dockyards. I can conceive no better use to which these structures could be applied, and no better way of isolating patients of this class for the purpose of treatment.

One word in conclusion as to the present condition of this metropolis as regards facilities for the treatment of women affected with venereal disease. I believe 200 beds would be an over-statement of the amount of accommodation afforded by the various hospitals for this purpose. This is, of course, ridiculously inadequate to the requirements of a population of from 2½ to 3 millions, and consequently the great majority of these cases are treated as out-patients at the different hospitals and dispensaries. Those admitted as in-patients rarely apply until their disease has reached a stage which renders it impossible for them to carry on their vocation any longer. Up to this period (I speak, of course, of professional prostitutes) they have been disseminating disease to the utmost extent of their opportunities. Consequently, the benefit which they themselves derive is reduced to a minimum, while the benefit which the public derive from their treatment is absolutely none at all; in fact, if they did apply at an early stage of their disease, which they very rarely do, they would stand no chance of admission, because the more urgent cases would always be preferred before them.

As regards out-patients, it is very doubtful whether they really derive much benefit from treatment, so long as they are compelled to follow their occupation; and in the case of prostitutes the alternative with them is either to do this or starve. Treatment only too often enables them to practise prostitution with less pain and inconvenience to themselves, and for a longer period, than they would otherwise be enabled to do.

I do not hesitate to express my conviction that, as regards the effect on the public health, it would be far better that prostitutes should not be treated at all, than be treated as out-patients, as so many of them now are. The only way to do good to them, and to prevent their doing harm to the community, is to take means to discover their disease, and to treat it as soon as it appears. This can only be done by compulsory periodical examination and compulsory detention in hospital. No amount of hospital accommodation maintained on the voluntary system will ever accomplish it. The women are far too careless of consequences, and the inconvenience which they suffer in the early stages is far too slight, to induce them to seclude themselves voluntarily for treatment. They will never of their own accord apply for admission until personal suffering compels them to do so.

Med. Chir. Rev. Jan. 1869.



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Various kinds of Writing employed by the same individual at different times.

ART. III.

On the Various Forms of Loss of Speech in Cerebral Disease. By H. CHARLTON BASTIAN, M.A., M.D. Lond., F.R.S.; Professor of Pathological Anatomy in University College, London; Assistant-Physician to University College Hospital, and to the National Hospital for the Paralysed and Epileptic.

THOUGH so much has been written upon this subject of late years, it is still one beset with difficulties of a most baffling description. Leaving alone the question of more precise localisation—as to the particular convolution which is to be looked upon as the seat of the "Faculty of Articulate Language"—which many now think of little moment on account of the *à priori* improbability of the existence of any such localisation, combined with the shifting and unsatisfactory nature of the evidence on which this view is based; there still remains the pressing difficulty, felt by all, of accounting for the fact that 'loss of speech' is much more frequently associated with lesions of the left, than with lesions of the right cerebral hemisphere. Are we to believe, with M. Broca[1] and Dr. Moxon,[2] that the left hemisphere is the one first and chiefly educated; with M. Baillarger and Dr. Hughlings Jackson[3] that both are educated, only that the left takes the lead in operations where both are concerned; or shall we confess that we stand before a mystery of which at present no adequate solution has been offered? These are still questions of the greatest interest open to discussion, but before taking them into consideration, there is, I think, also room for inquiry whether we have yet arrived at a correct appreciation of the relation of the various forms of loss of speech to one another, and whether hitherto all the phenomena of 'Aphasia' have been satisfactorily explained and harmonised with what we know concerning the physiology of Thinking as a process dependent upon language. Now I venture to think that hitherto the explanation of several of the phenomena presented by aphasics have not been satisfactorily explained, owing to an erroneous view which has been prevalent concerning the physiology of thinking; and still further, that if the views which I entertain[4] as to this latter process be correct, we then have it in our power to explain some of those points of which the explanation hitherto has not been adequate.

The first part of our task will consist, therefore, in an inquiry as to the relations existing between the several forms of 'loss of 1. speech' in cerebral disease, combined with an attempt to explain the phenomena presented in the various classes in accordance with the doctrine of the physiology of spoken thought, which holds that words are revived in the cerebral hemispheres as remembered Sounds, which when revived in an unconscious and automatic 2 manner, call into play through the intermediation of the requisite volition the various muscles necessary for the articulation of the sound. This doctrine is advanced in opposition to the one stated by Professor Bain and others, that words are necessarily "motor processes,"[1] and that "a *suppressed articulation* is, in fact, the material of our recollection, the intellectual manifestation, the *idea* of speech."[2] Having gone into the evidence bearing upon this question elsewhere,[3] I will not now recapitulate; and seeing how often of late the story has been told of the gradual growth of opinion as to the localisation of the so-called "faculty of language," I need only refer the reader to some of the best of these.[4]

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[1] 'Bullet. de la Soc. d'Anthropol.,' t. vi, (June) 1865.

[2] This 'Review,' April, 1866.

[3] 'Med. Times and Gaz.,' June 23, 1866, and Sept. 26, 1868.

[4] For a statement of these see the current number of the 'Fortnightly Review.'

Loss of speech, in its partial forms, is associated with affections of the brain of all degrees of gravity; frequently it is owing to the mere failure of memory for words or names; and it may be due to such general conditions as over exhaustion from excessive mental or even physical exertion, or to the occurrence of some febrile illness; whilst on the other hand it may be only one symptom of a grave structural disease of the brain, which has also produced hemiplegia, or some other well-marked paralytic condition. But it is also true that loss of speech in all its grades up to complete inability to utter any distinct articulatory sounds, may exist without any motor paralysis whatever of the hemiplegic kind.

The relations of the various forms of loss of speech to one another will be most easily appreciated if I arrange them at first in some such tabular form as the following, and then proceed to speak of each variety with more or less detail, devoting more particular attention, however, to the various forms which have been included under the name 'Aphasia,' and alluding to the others only in so far as they serve to throw light upon these more obscure conditions.

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[1] Dr. J. Hughlings Jackson, in 'Med. Times and Gaz.,' Sept. 26, 1858.

[2] 'Senses and Intellect,' 3rd edit. p. 336.

[3] Loc. cit.

[4] Trousseau's 'Lectures,' translated by Dr. Bazire, Pt. I., p. 240; Dr. Bateman's papers on "Aphasia" in 'Journ. of Mental Science,' 1867; and Dr. Sanders in 'Edin. Med. Journ.,' March, 1866.

- LOSS OF SPEECH
  - Ability to articulate unimpaired.
    - 1. Absence of wish to speak, *e.g.*, in some cases of insanity, and perhaps of hysterical aphonia.
    - Wish to speak existing
      - 2. Various forms of paralysis and incoordination of memory. *Amnesic defects.*
      - 3. Memory existing, and words being revived as *remembered sounds*, the mechanism by which these incite the automatic acts of speech is interfered with. *Ataxic defects.*
  - Ability to articulate impaired.
    - 4. Slightly, from paralysis of ninth nerve, &c., in cases of hemiplegia, with or without co-existing 'aphasia.'
    - Gravely . .
      - 5. In some severe cases of hemiplegia, either with or without co-existing 'aphasia.'
      - 6. Advanced cases of glosso-laryngeal paralysis.

ABILITY TO ARTICULATE UNIMPAIRED.

1. It is by no means a very uncommon thing to meet with insane people in asylums who have not been known to speak a single word for years. They are generally more or less demented or melancholic, though it seems obvious that there is no actual inability to speak, since from time to time they do exercise this power. One instance came under my observation, where a man had not uttered a single word for some years; he then had a slight attack of pleurisy, and whilst he suffered from the pain of this affection, for two or three days, he talked pretty freely, and answered all questions addressed to him. On another occasion, whilst suffering from the pain of toothache, he spoke and answered questions for a time, but after the extraction of the tooth at once relapsed into his usual silent and stolid condition. Dr. Forbes Winslow mentions[1] the case of a man who had been insane for fifty-two years, and had not spoken for *thirty* years, who recovered the use of his speech about fifteen days before his death, and "answered perfectly well all questions put to him." These cases, and also some of those of so-called hysterical aphonia, would seem to be due rather to the want of the necessary volition or will to speak than to any inability to do so, and would therefore represent rather the absence of the first physiological condition necessary for spoken thought.

2. Passing on now to the second class of cases; in these we find that the wish to speak does exist, only that its execution is impaired by reason of some defect of memory; which defect of memory also, inasmuch as it prevents the words being revived in consciousness as

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[1] 'Obscure Diseases of the Brain,' 3rd ed., p. 485.

they should be, impairs the process of thinking almost as much as it does that of speaking. In the acquirement of a language, there gradually grows up an association in the mind between certain things or mental states and certain articulate Sounds corresponding to the Names which have been given to these. And in the process of thinking, so long as the memory be good the words become nascent in consciousness as revived sounds, and these bring about, in a reflex or automatic manner (in conjunction with a stronger or weaker volitional effort), those combinations of muscular action necessary for the articulation of such sounds. Well, then, if this *primary* memorial association between things or mental states and their names be defective, then it is evident that in proportion to the degree of the defect must there be a diminution at once, not only of our power of speaking, but also of thinking.

It is observed, that in the slighter forms of loss of memory, it is only the more special kinds of association which cannot be recovered, such as the names of persons or of places; whilst in the more severe cases *substantives* generally may be incapable of being recalled to memory; after these *verbs* disappear, and last of all *adjectives*, which have a more general signification than any of the other parts of speech.

There are two subdivisions of the defects of memory which deserve to be distinguished in this place. One in which the defects are of a *paralytic* kind, due to more or less of failure of action in the parts of the brain concerned; and another, in which the defects are rather dependent upon *irregular and improperly co-ordinated action* in the same parts. Now it will be found that the so-called *amnesic* forms of aphasia group themselves principally as defects of memory of this latter kind, and only some of the paralytic forms have any very special bearing upon the question of loss of speech. These I will first enumerate.

Paralytic Defects of Memory. *a.* Momentary forgetfulness and confusion about proper names and nouns, though power of recovery after a time; or a more permanent and habitual forgetfulness of names, of objects, persons, or places, and attempts to remedy this forgetfulness by employing a periphrasis in place of the noun which cannot be recalled.

b. Sudden and momentary loss of the "thread of thought."

c. Inability to continue any train of thought at all after the slightest interruption.

These paralytic defects of memory may take part in producing some of the so-called *amnesic* forms of 'aphasia,' but then such defects are generally conjoined with other anomalies, such as are now to be mentioned.

Incoordinate Defects of Memory. *a.* The person cannot recollect the words in which he wishes to express himself, and is conscious of his mistakes when he employs the wrong ones. This forgetfulness is generally more especially marked in respect of *nouns*, or names of objects.

b. There is the same forgetfulness of words; and the individual is, moreover, unconscious that he makes use of wrong words.[1] In both these classes of cases it may be found that the word or *sound* uttered is associated in some way with the word which should have been employed. And as Dr. William Ogle remarks, although frequently this association is one of sight, it is still more frequently one of sound. Where no connecting link of this kind exists, it is probably indicative of a graver impairment of the memorial faculty.

c. Although the person recollects the words suitable for expressing his thoughts, they are uttered without any proper order or sequence, so that his meaning is unintelligible. Dr. Winslow records a remarkable instance of this rare kind of defect. It was met with in a gentleman who had partially recovered from an apoplectic attack. "He could speak; but what he said, without a key to its interpretation, was quite unintelligible. He was able to pronounce words with great clearness, but they were sadly misplaced and transposed. What he said was written down, and the words [could then be] placed in their proper order. By adopting this course his family were able clearly to comprehend his wishes. This state of brain and impairment of speech continued with slight intermissions for nearly a fortnight, accompanied by acute pain in the occipital region."

d. Ability to use certain verbal forms only combined with forgetfulness of others, *e.g.* Dr. Winslow speaks of a lady who after an attack of paralysis "only used the infinitive of verbs, and did not employ any pronouns. For example, instead of saying 'I wish you good day; stop, my husband has just come,' she would remark, 'To wish good day, to stop, husband to come." This patient eventually succeeded in recollecting pronouns, though she still failed in making a proper use of them.

e. Inability to pronounce words in the ordinary manner, which instead are spoken as sounds corresponding with some transposition, more or less regular, of the several letters of which each word is composed. Thus Dr. Winslow mentions[2] the case of a man who after an attack of paralysis always transposed the letters of words, so that "endeavouring to say the word *flute*, he said *tufle*, *puc* for *cup*, *gum* instead of *mug*."

f. Inability to pronounce words properly, owing to the substitution of certain letters for others almost invariably; *e.g.* a friend of Professor Gruner of Jena is said to have always substituted a *z* for an *f* in every word which should have contained the latter letter.[3]

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[1] M. Trousseau records a very remarkable instance of this inability to comprehend the words which were made use of. A lady rose with an air of kindness to receive a visitor, and pointing to an arm-chair, said, "Cochon, animal, fichue bête!" whilst her son-in-law, who was present, and knew what she really meant, said, "Madame vous invite à vous asseoir,"—the lady all the time seeming quite unconscious of the insulting expressions she had used. Luys, speaking of similar cases, alludes to an instance where the person was continually in the habit of using one word for another, without being conscious of his mistakes. One day he pronounced the word "jardin" wishing to say "lit," repeated it several times, and then fell into a violent passion because his orders were not comprehended. He was then made to write the word that he wished to make use of, and the *sight* of the proper written symbols soon convinced him that the word which he had uttered was not the one which he had intended to utter. He then made ample apologies to those with whom he was previously angry, and freely admitted that he alone was to blame. See also a case recorded in note at p. 231.

Luys' view as to the way in which such a defect is to be explained may be gathered from the following words:—"Il faut en un mot que, sous peine d'écarts véritablement *ataxiques*, il ait la notion auditive *récurrente* de ce qu'il a dit et des effets vocaux qu'il a voulu produire." ('Système Nerveux,' Paris, 1865, p. 395.)

[2] 'Loc. cit.' p. 315.

[3] Inquiry into 'Mental Derangement,' by A. Crichton, M.D., Vol. I., p. 373.

Of these anomalies of memory, due to incoordination, the first only is common, the other varieties (with the exception of the second, which is not so rare) are very scarce, and are remarkable as much from their rarity, as on account of the strange nature of the defects. Various degrees of either of the first two kinds of defects, alone or in conjunction with a certain amount of paralysis of memory such as I have previously spoken of, when existing, constitute a kind of 'Aphasia' which was clearly recognised by Trousseau, and by most subsequent observers, and for which Dr. Sanders proposed the name of *amnesic aphasia.*[1] In my opinion, the thinking power of the individual is impaired in these cases almost in direct proportion to the loss which he experiences in his powers of expression, the only set-off in favour of his power of thinking being the fact, that if it is the name of an object which he is unable to recall, an *idea* of the thing may be capable of being recalled to mind in the form of a visual conception, when its name is incapable of being revived in the auditory 'Perceptive centres,'[2] and therefore incapable of being realised in the ordinary way during the thinking process.[3] I think these cases should not be included under the name of 'Aphasia' at all. They belong to a thoroughly distinct class, and I think we should do well to reserve for this the name AMNESIA VERBALE; or, for the sake of brevity, even AMNESIA simply. In order to illustrate this condition, I give an abbreviated account of a remarkable case recorded by Dr. Banks,[1] in which amnesic defects of both speech and writing were exhibited in an extreme degree.

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[1] In his paper, in the 'Edin. Med. Journ.' for March, 1866, he pointed out the distinction between what he termed the *amnesic* and the *ataxic* forms of aphasia,—a distinction and nomenclature closely agreeing with that subsequently advanced by Dr. Wm. Ogle.

[2] For the meaning which I attach to this phrase I may refer to a paper in the current number of the 'Journ. of Ment. Science.'

[3] Dr. William Ogle ('St. George's Hosp. Repts.,' 1867, p. 94) in his paper on "Aphasia and Agraphia," says:—"Now a similar forgetfulness of words but more extensive—a similar inability that is to translate ideas into symbols—constitutes one form of aphasia; a form which I will call *amnemonic aphasia,* to distinguish it from another variety to be mentioned presently. The patient, in such a case, suddenly in speaking stops short, unable to recall the word he requires. The moment, however, that he is prompted, he is able to go on." Now, it seems to me, when Dr. Ogle says, with reference to this form of aphasia, "the idea is there," only it "does not suggest the proper symbol," that this is only partially true, and does not at all adequately indicate the extent to which the power of thinking is impaired. Since the symbols we ordinarily make use of in thinking are those which can only be revived in the auditory perceptive centres, I think if these are not forthcoming it can scarcely be said that the "ideas" or materials of thought come into existence at all adequately. It is true, as above indicated, that in the case of the forgetfulness of certain words or names, *pictorial* images or conceptions may, to a certain extent, take the place of the auditory symbol, and so enable the individual to realise his thoughts. But how can visual images assist him if his thought halts from forgetfulness of such words as "perjury" or "virtue?" Those forms of defective power of expression where the words seem to be half present to consciousness, and yet cannot be articulated, though they appear to be "on the tip of the tongue," do not, I think, belong to the category of which I have just been speaking, but rather fall into the next.

A gentleman, aged about seventy-five, whose previous health had bee excellent, began some years since to show signs of senile decay. About fou years ago he had a slight apoplectic attack, which soon passed off withou leaving any form of paralysis.

On the 28th of March, 1864, after having walked a considerable distance during the day, he sat down to dinner, and proceeded with his meal as usual. But after a time it was observed that some of the water he was drinking flowed from his mouth, and he put down the glass, calling at the same time in a loud and excited voice for his wife and the servant who was in the habit of attending to him, notwithstanding that they were present. The patient was seen by Dr. Kidd in a very short time. He was then sitting on the sofa, looking puzzled, but evidently conscious, calling out loudly at intervals for the servant and others, but not taking the slightest notice of anything which was said to him. The excitement under which he laboured after a time passed away. When a mustard sinapism was applied to the back of his neck, in a short time he put his hand to the place and complained of its being "very toast," and also endeavoured to speak, but unintelligibly, on other subjects. He walked upstairs unassisted, wound up his watch, then went to bed and slept well. The following morning he recognised Dr. Kidd, but failed in naming him. It was now found that he was completely deaf, the loudest noises not being perceived by him. He could see perfectly, and there was no paralysis of any kind. In speaking he used wrong words, so as to be utterly unintelligible. He called for his "cup," meaning his wife, and he applied the word cup in the most diverse and extraordinary ways. Dr. Banks then saw him, and he says, "he certainly recognised me, and was glad to see me, but misnamed me; saying something, but using words without meaning. We endeavoured to communicate with him by writing, but it was evident that he did not understand it. 'Have you pain?' was written, and he looked at it and said, 'Good, good God;' appearing to read what was written." At the beginning of April a remittance was due from his agent, and each morning he was much excited, asking frequently for something. At length it occurred to one of the family to show him his agent's letter, which seemed to please him; but he was not quite satisfied till the money was brought and counted before him. Some shillings were not shown to him at first, but when he saw them he appeared to know all was right, and then, on the money being handed to his wife, appeared content. His feelings of affection for his wife seemed to be intensified; but there was some amount of emotional weakness. He attempted to write letters frequently, and his address was written twice or three times at the head of the sheet of paper, some of the words being imperfect. "My dear Sir," was written correctly. The sheet was filled with writing, but no word except "wife" was legible, the rest being utterly meaningless; some letters were correctly formed, but no words until the end, where his name was signed with a steady hand and in his usual manner. He varied, however, in his power of writing at different times; occasionally when wished to sign his name, he could not be induced to do so, and "only scribbled some unintelligible words." It was impossible to get him to understand anything; and his meaning could only be guessed at by his gestures, and by the very few words at his command, which were almost always misapplied. He occasionally for a time made much use of some one word, applying it in the most varied ways. Wishing to inform Dr. Kidd that a liniment which he had been using was nearly finished, he said, pointing to the bottle, "Bring the cord." On another occasion, speaking of pills he had been taking he said he had taken "potatoes." Very frequently there was some similarity in the word used to the right one; or it could be discerned that there was some association with the idea he wished to convey; for example, giving his waistcoat to be put aside, the watch being in the pocket, he said—"Take care of the break-fall." Once, on directing attention to his side, probably the seat of pain, he said—"Oh! there is a world here." He seemed conscious of his deafness, and sometimes spoke of it. One day he said he could neither hear nor read—"Only a little, could read the words, but could not take in the meaning." Every morning, notwithstanding, he spent some time as if busily engaged reading the Bible and the newspapers. This was, doubtless, from the mere force of habit; for on testing him, he read after a fashion, but the words were unconnected and meaningless, and had not even the most remote connection with the text. His powers, both of speaking and of writing were subject to variation at different times. (Lithographs of two letters are given by Dr. Banks which, though made up of properly written words, are almost utterly unintelligible.) Occasionally it was difficult to manage him, as he frequently wished to go somewhere; and when it was impossible to understand him he became very much excited. He continued in much the same condition till the 7th of October, when he had a distinct apoplectic attack, and became completely hemiplegic on the right side of the body, though there was no paralysis of the face. He only lived a week after this attack, and no particulars are given concerning his mental state during this time.

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[1] 'Dub. Quart. Journ. of Med. Sc.,' Feb., 1865, p. 73.

Concerning this case it should be observed that the deafness was altogether an exceptional occurrence, and one somewhat difficult to explain also. In this as in many other cases where amnesia exists, there was no motor paralysis of any kind. The same condition may, however, co-exist, with distinct hemiplegia, an instance of which—showing also a slighter form of this anæmic defect—is recorded at p. 232 of this paper.

3. We now come to a class of cases of most remarkable interest and of great variety—those which have been more especially included under the term 'Aphasia.' But before commencing a consideration of them I would call the reader's attention to some of the phenomena involved in Thinking and in Speaking. The names or words in which thoughts are expressed become nascent contemporaneously with the thoughts—we *think in words*,[1] in fact, and these words are, I believe, revived as sound impressions in the auditory perceptive centres of the cerebral hemispheres. This alone is necessary in order that a process of silent thought should be carried on—the memory being good; but when this thought has to be spoken articulately, then, as I hold, it is necessary that the nervous mechanism should be intact, by means of which the primary motor impulses (reflected from these revived auditory impressions) are transmitted downwards from the grey matter of the cerebral hemispheres to the more immediate motor centres for speech—these, perhaps, being partly situated in the corpora striata, and partly in the medulla oblongata. If this passage or line of transmission is imperfect, then, although *thought*

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[1] That is to say the *idea* and its symbol or word having so frequently been called up together, as to have become at last, as it were, organically fused and inseparable in ordinary thinking.

may proceed in a more or less perfect manner, it is not possible for the individual properly to translate this thought into acts of articulate speech. Then, also, with regard to the physiology of Writing, it must be borne in mind that, although the individual thinks, previously to writing the actual words, in the ordinary way (the word being revived in thought as a sound perception), when he does begin to write them, in all probability the acquired and now more or less automatic muscular acts needed for the writing of the words, are rather immediately incited by revived visual impressions which have been called up by the previously revived sound impressions, than by the sound impressions themselves. So that whilst in speech the motor acts are immediately dependent upon revived sound impressions, in writing the motor acts are only mediately dependent upon these, and are the immediate results of the revived visual impressions of the letters, which the corresponding sound associations have called up. Thus, although the two processes of speaking and of writing are to a certain extent processes which have steps in common with one another, still they do differ to such an extent that it is quite conceivable for the one power to be affected whilst the other may remain almost intact; and this is sometimes the case, although in the majority of instances, impairment in the power of speaking and of writing are met with in the same individual.[1] These considerations, in fact, suggest certain primary divisions of this part of our subject, by adopting which I think we shall be able not only to treat the question more satisfactorily, but also to bring into due prominence what I consider to be natural varieties met with in the various cases of loss of power of expression.[2] These divisions which I would propose to name respectively APHASIA, APHEMIA, and AGRAPHIA, may be defined, generally, as follows:[3] —

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[1] It was from observation of these varieties in the mode of association of defects of writing and speaking, that Marcé was led to believe there must be a separate centre of co-ordination for each of them (Mém. de la Soc. d'Biolog., 1856).

[2] I adopt the more general phrase here, since division III. could scarcely be ranged as a variety of "loss of speech." Such an impairment, however, may exist to a slight extent even in these cases, and obviously the two kinds of defect require to be spoken of in connection with one another.

[3] It will be seen, thus, that whilst I reserve the name *Aphasia* as the designation of the condition to which this word has been more especially applied, I narrow its signification by resuscitating Broca's original term *Aphemia*, as a proper and fitting designation for one class of cases which has hitherto been included under the term 'Aphasia,' and by proposing for another group of cases which has hitherto been similarly included under the one term, the more fitting appellation of *Agraphia*. Of course *aphasic* patients are, by virtue of this condition, at the same time both aphemic and agraphic; but the individuals of the *aphemic* division are characterised specially by the fact that they *cannot speak*, though they can write, whilst those of the *agraphic* division are distinguished by their *inability to write*, whilst the power of speech remains. These three terms, together with that of *Amnesia*, enable us clearly to distinguish from one another all the cases which have been hitherto grouped under the one name 'Aphasia.' A classification

I. APHASIA including persons who can *think,* but cannot *speak* or *write.*

II. APHEMIA including persons who can *think* and *write,* but cannot *speak.*

III. AGRAPHIA including persons who can *think* and *speak,* but cannot *write.*

I.—APHASIA.

Under this first head are to be included by far the largest number of cases which have hitherto been spoken of under the names 'Aphemia' or 'Aphasia;' those falling into other two divisions are rather rarities of a most interesting kind, only occasionally to be met with.

As before stated, the fact of the coexistence with such cases of a more or less well-marked hemiplegic *paralysis,* is to be looked upon in the light of complication (though a very frequent one), since the most typical cases of this defect may exist without any ordinary motor paralysis whatever.

Then, with regard to the degree of general *mental impairment* existing in aphasia, there cannot be a doubt that here also the widest latitude of variation is met with. Trousseau has gone carefully into this question, and he came to the conclusion that, although in many a certain amount of weakening of the intellect was easily appreciable, in others it became much more difficult to ascertain the existence of any general failure in mental power. These, however, are the rare exceptions, and in a certain number of other cases the dementia is well marked, and may vary in degree up to an almost complete abolition of all mental faculties whatever.

The *powers of articulation* and of speech remaining to these patients are also very various, and may be generally ranged under one or other of the following heads:—

a. Cannot utter a sound, or only mutters inarticulately.

b. Constantly repeats some meaningless sound or sounds, such as "*tan-tan,*" or "*cousisi.*"

c. Uses some single word, such as "*yes;*" and also mere sounds, such as "*ba-ba,*" or "*poi-boi-ba.*"

d. Uses some short phrase habitually, such as "*List complete,*" or "*I want protection.*"

e. Uses three or four words or expressions—though in themselves meaningless or irrelevant—in a constant and definite manner; as in Broca's case (Lelong) in which the words "*oui,*" "*non,*" "*trois,*" and "*toujours*" were so employed.[1]

In general, at the same time that the patients present these pecu-

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of this kind is, I think, needed, and if we clearly understand the signification given to our terms, we had perhaps better employ those which have already been used, rather than fastidiously invent new ones.

[1] 'Bulletins de la Soc. Anatom.,' Nov., 1861.

liarities as to the sounds or words that they habitually make use of, they are unable to repeat even the simplest sound which has just been uttered before them as a copy. In other cases, however, they can repeat single words or vowel sounds, which they have just heard slowly articulated; whilst in a very few cases a remarkable power of mimicry is shown. Thus, Dr. Bateman[1] speaks of a woman seen by him at the Salpêtrière, aphasic and paralytic on the right side, who mimicked everything that was done and repeated everything that was said, although unable to utter a single word spontaneously, and of her own accord.[2]

It has been pointed out by several writers, but most strongly insisted upon by Dr. J. Hughlings Jackson, that many aphasic patients are able to utter additional words under the influence of some strong *emotion*—not unfrequently taking the form of swearing—although they are quite unable to repeat such words or phrases immediately afterwards, by means of a simple volitional effort, however strong.[3]

Patients whose powers of speech are thus limited, as a rule, cannot *read aloud* in the least, either from a printed book or from a manuscript, though they do understand more or less what is spoken, or what is read to them, and in many cases they can themselves read books silently, so as thoroughly to understand them. Some few cases, however, are on record in which it was found that the patient could read aloud, though at the same time unable to speak in other ways. One instance of this is recorded by Dr. Hertz,[4] who says :—

> "In August, 1785, I was called to an officer of the artillery, a man about forty years old, who, as I was informed, was seized with a palsy in consequence of cold and violent anger. His tongue, hands, and feet were paralysed by the attack. He was under the care of one of our first physicians, at whose desire I was consulted concerning the propriety of applying electricity. From the time that this remedy was first applied till the following year I never saw him; but he then sent for me again, as his own physician, he said, had deserted him.

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[1] 'Med. Times and Gaz.,' Sep. 5, 1868.

[2] A case has also been recorded by Trousseau (loc. cit., p. 226), where this mimicking power existed to a slighter extent. The only two expressions which the man was in the habit of making use of himself were: "Ma foi!" and "Cré nom d'un coeur!" With these phrases he generally answered all inquiries; but, on one occasion, being asked "Are you not from Haute Loire?" he replied, like an echo—"Haute Loire." "What is your name?" "Haute Loire." "What is your profession?" "Haute Loire." "But your name is Marcon?" "Yes, Sir." "You are sure your name is Marcon?" "Yes." "What is your country?" "Marcon." "Not at all, that is your name?" And then, with a gesture of impatience—"Cré nom d'un coeur."

[3] Cases have been recorded by Dr. Jackson, Dr. Gairdner, Dr, Robertson, and others, in which aphasic individuals had preserved their musical faculties so far as to be able to sing—of course without words. The husband of Dr. Gairdner's patient, speaking of his wife, says: "She had a very fine ear for music, and now, though she cannot utter the words, she joins in our family worship, and follows the tune as correctly as ever she did, taking every note, and sounding it sufficiently loud to enable us to hear the melody from her."

[4] Quoted by Dr. Winslow, from 'Psycholog. Mag.,' vol. viii.

I found him so much recovered as to have the complete use of his feet; his hands also were stronger, but in regard to his speech the following very remarkable circumstance was to be observed: he was able to articulate distinctly any words which either occurred to him spontaneously, or when they were slowly and loudly repeated to him. He strenuously exerted himself to speak, but an unintelligible kind of murmur was all that could be heard. The effort he made was violent, and terminated in a deep sigh. On the other hand, *he could read aloud with facility*. If a book or any written paper was held before his eyes, he read so quickly and distinctly that it was impossible to observe that there was the slightest fault in his organs of speech. But if the book or paper were withdrawn he was then totally incapable of pronouncing one of the words which he had read the instant before. I tried this experiment with him repeatedly, not only in the presence of his wife, but of many other people: the effect was uniformly the same."

Since this case is so exceptionable, I think it will be well to place after it another possessing many points of resemblance. It has been recorded by Dr. Hun,[1] but I cannot do better than avail myself of the abstract of the case which has lately been given by Dr. Bateman ('Journal of Mental Science,' April, 1868):

"Dr. Hun, of Albany, mentions the case of a blacksmith, æt. 35, who, before the present attack, could read and write with facility, but who had been labouring for several years under a disease of the heart. After a long walk in the sun he was seized in the evening with symptoms of cerebral congestion, remaining in a state of stupor for several days. After a few days he began to recover from this condition, and understood what was said but it was observed that he had great difficulty in expressing himself in words, and for the most part could only make his wants known by signs. There was no paralysis of the tongue, which he could move in all directions. He knew the meaning of words spoken before him, but could not recall those needed to express himself, nor could he repeat words when he heard them pronounced; he was conscious of the difficulty under which he was labouring, and seemed surprised and distressed at it. If Dr. Hun pronounced the word he needed, he seemed pleased, and would say, "Yes, that is it," but was unable to repeat the words after him. After fruitless attempts to repeat a word, Dr. Hun wrote it for him, and then he would begin to spell it letter by letter, and after a few trials was able to pronounce it; if the writing were now taken from him he could no longer pronounce the word; but after a long study of the written word, and frequent repetition, he would learn it so as to retain it, and afterwards use it. He kept a slate, on which the words he required most were written, and to this he referred when he wished to express himself. He gradually learned these words and extended his vocabulary, so that after a time he was able to dispense with his slate. *He could read tolerably well from a printed book, but hesitated about some words. When he was unable to pronounce a word he was also unable to write it till he had seen it written;* and then he could learn to write as he learned to pronounce, by repeated trials. At the end of six months, by continually learning new words, he could make himself understood pretty well, often, however, employing circumlocution when he could not recall the proper word, somewhat as if he were speaking a foreign language imperfectly learned."

I will not attempt anything like explanation of these remarkable cases at present, but shall refer to them again further on, when endeavouring to give some physiological elucidation of the phenomena presented by aphasic persons generally. It is, unfortunately, not stated with regard to the first of these cases, whether the individual was able to write, but it is especially worthy of note that in the second case the power of reading aloud seemed pretty clearly co-extensive with the power of writing.

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[1] American 'Journal of Insanity,' April, 1851.

If we next consider the capability which aphasic persons have of expressing themselves in *writing*, we find that here also different individuals do not by any means possess a like power, and that the differences in this respect closely resemble the various capabilities of speaking that have been already enumerated. It is by no means invariably found, however, as we have just seen, that the same patient possesses an equal capability of expressing himself in both directions.[1] The following varieties have been noted:—

a. Can only make mere meaningless strokes.

b. Can form separate letters, but cannot combine them in a suitable manner into words.

c. Can write his or her own name, or some other very familiar word without a copy.

d. When a polysyllabic word is dictated can write the first syllable, though the rest is generally all confusion. Or can write a single word, such as "London," alone, but fails to write more than the first syllable of this when told to write "London is very big,"—commences, looks puzzled, and throws down the pen in despair.

Very many patients can copy words which have been written before them; indeed, this is generally found to be much more easy of accomplishment than the somewhat parallel process of repeating words they have just heard slowly uttered.[2] Many patients can also write words when they have a printed copy before them, realizing the necessary change mentally from the printed to the written symbol during the execution of the act.[3] For the most part, however, they are quite unable to write from dictation, though strange exceptions even to this rule have been met with. Dr. Osborn alludes to one recorded in 'Ephemerides Curiosæ,' in which an elderly man, after an attack of apoplexy, forgot how to read, or even to distinguish one word or letter from another; but if a name or phrase was mentioned to him he could write it immediately and accurately. He was, however, incapable of reading what he had written, and if asked what a letter was, or how the letters were combined, it became evident that the writing had been performed *automatically,* without any exercise of the reflection or judgment. In this case none of the means that were employed were successful in restoring the knowledge of letters to his mind. Marcé[1] also records a case of an attorney who was quite unable to read, who could not express his ideas at all in writing, but who wrote without difficulty any word just pronounced before him, although at the same time he remained quite ignorant of its meaning.[2]

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[1] Of course, in testing the power of writing, it should always be ascertained, as far as possible, what were the individual's powers in this direction previous to the attack; and if the patient be also paralytic on the right side, he has to be taught the manipulative process of writing with his left hand, before we can judge as to how far he is able to express himself in writing.

[2] Marcé records one of these cases (loc. cit., p. 110) in which the singular peculiarity was noticed, that the man was able to write figures with much greater precision and ease than the ordinary characters of writing. And M. Lasegue knew a musician completely aphasic, who, being unable to speak or write in the ordinary way, was, after hearing a passage of music, able to write it on paper with ease.

[3] When a person, meaning to write one word writes another, or a word that is only an approximation to the one intended, this defect is probably due to, and seems generally to be associated with, the ataxic defects of memory, which are met with in the forms of *Amnesia* previously referred to.

Having stated generally the nature and extent of the defects in the various modes of expression presented by APHASIC persons, it will now I think be necessary for me to show in what way these various defects are actually grouped in cases coming under our observation. This I think can be best done if we throw the cases belonging to this division into four groups, comprising—*a, very severe; b, severe; c, typical;* and *d, slight* cases, exhibiting this defect, and illustrating each group by the details of some case which may be considered to represent it appropriately. Of course it must be understood, however, that such a classification is a purely arbitrary one, and that although the great majority of cases occurring might be ranged under the one or other of these groups, still that others do exist which should occupy almost every conceivable intermediate position. Just as it would be impossible strictly to classify the lesions giving rise to the morbid symptoms, so as to be able to find that none ought rather to occupy intermediate positions of the classified series, so we are unable to classify definitely all the groups of symptoms to which these various lesions give rise.

a. Very severe. The amount of mental impairment is generally so great in these cases, that inability to speak is only one of its modes. These very severe cases are almost invariably associated with recent hemiplegia, and the patient may, in the course of two, three, or more weeks, pass out of this very grave stage, and then present such an assemblage of symptoms as would find a place rather in our second or third groups. As an instance of this very grave form of the defect I give the following particulars concerning a case which was admitted into University College Hospital under my care, on October 5th, 1868:—

M. C—, æt. 34, married, and has had three children, but has suffered much mental distress lately owing to the death of one of these. On October 3rd had a fit for the first time, whilst in the street, but was able to walk home, and between this period and time of admission she had twelve other epileptiform attacks. She was found to have no valvular or other cardiac disease, and there was no history of syphilis.

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[1] Loc. cit., p. 103.

[2] In possible explanation of these cases, one is tempted to ask, whether the persons had been much accustomed to write from dictation, as certainly in the second case the attorney might have been, in taking down evidence. In such a case the act of writing would come to be more than usually associated with auditory impressions. These cases, however, will be referred to again further on.

Soon after admission she had another series of epileptiform convulsions, affecting both sides of the body, though principally the right, and twitchings also of the right side of the face. During the intervals between these convulsive attacks, it was found that the face was partially paralysed on the right side, the right arm was completely paralysed, and the leg to a less extent. She had had six of these attacks during the three days after admission, and during this time remained in a dull, lethargic state, with eyes half opened, but taking no notice when spoken to, or of things going on around her. Not lying still, however, but restless, and with a slow pulse—56 per minute. She continued in much the same state till the 13th of October, when the pulse altered in character, becoming much more frequent, and the patient gradually began to regain a certain amount of intelligence in look and manner. On the 19th, her attention could be at once arrested; she made decided efforts to reply to questions, and was able to say "yes" and "no" indistinctly, though not appropriately. When told to show her tongue she opened her mouth, but did not attempt to protrude the organ. Pulse 88. Was able to swallow without difficulty, and took food eagerly. On the 26th, seemed still more intelligent. Did not protrude tongue when told, but opened her mouth and took hold of it with her fingers with the view of bringing it forwards. Although unable to move it by a mere volitional stimulus, on a sweet lozenge being applied to her lips she immediately put out her tongue with great readiness, and whilst eating it laughed and seemed much pleased. On the 28th, looked still brighter, and took notice of what passed around her. Made signs when she wished to attract the attention of nurse. When asked if she had pain in the head, she nodded assent, but did not move her hand when told to place it upon the painful part, or else moved it in quite a different direction. Paralysis of limbs and face continued much the same.

About ten days afterwards I examined her again carefully. She had continued to improve in the meantime, and could now say "Nurse" distinctly, in addition to "Yes" and "No." She could not repeat the simplest vowel sounds, neither could she read single words in large print, either aloud or to herself, so as to comprehend them. She could not, in fact, point out individual capital letters of very large type. When asked to point out M, after a long time, and much pressing, she placed her finger upon W; and when told she was not right, and asked to point out W, after a still longer interval, she laid her finger upon S. She seemed to recognize familiar objects, and know when the right name was given to them. She could not be made to count at all by tapping with her fore-finger, after having been shown in the most deliberate way. She could not be induced to give a single tap, and only looked distressed. She seemed to recollect her own name. And although she did not give any signs of recognition when the name of the street in which she lived was mentioned, she immediately nodded assent when she heard the remaining part of her address, viz. "Fitzroy Square," pronounced. She rarely laughs, but frequently has fits of crying. She utters no additional exclamations when excited, and her vocabulary is confined to the three words above mentioned.

b. A case very suitable for illustrating this group has been recorded by the late Dr. Bazire,[1] in reference to Trousseau's statement, that there was an unaccountable and frequent absence of that emotional weakness in persons who are aphasic, although such weakness does display itself most commonly in patients who are in the same way hemiplegic, though not aphasic. Dr. Bazire thought this emotional weakness did manifest itself in cases of aphasia with paralysis, not at all rarely, when the degree of hemiplegia was indicative of an extensive brain lesion. The condition showing itself "not only by a greater proneness to shedding tears than the occasion warrants, but by a greater tendency also to immoderate laughter, and to a rapid, sometimes sudden and unexpected, transition from laughter to tears." My own experience entirely accords with this. I have seen some notable cases, and have two under my care at present, in which well-marked emotional weakness co-exists with aphasia and hemiplegia.[1] I am disposed, therefore, to think that too much stress should not be laid upon this distinction which Trousseau attempted to establish. Subjoined is Dr. Bazire's case, almost in his own words, but abbreviated.

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[1] Translation of Trousseau's 'Lectures on Clinical Medicine,' Part I, p. 264.

M. W—, æt 24, a young woman of short stature and pale complexion, but apparently well nourished, was admitted an out-patient at the National Hospital for the Paralysed and Epileptic, on January 10th, 1865, suffering from imperfect right hemiplegia, and complete aphasia. To all my questions she invariably answered, "Sapon, Sapon." It was ascertained from a relative who accompanied the patient, that she had been seized with paralysis on the right side three months previously. She had led an irregular life, but whether she had had syphilis could not be ascertained. Having only suffered from severe headache for a month previously the actual attack was sudden. She dropped down senseless, remained in a comatose condition for several (?) days, and when she recovered her senses could not utter a single word beyond "Sapon, Sapon," which she has ever since kept repeating at every turn. The paralysis was not complete after the first few days, and her face is said to have been involved.

When I first saw her the patient had walked to the hospital, a distance of about two miles from her residence. She dragged the right leg much in walking and was unable to stand on it alone. She could move her right arm, but not so as to use it, and she could not squeeze my hand at all. There was no rigidity, no wasting of the limbs. The facial paralysis had disappeared, for the patient could show her upper teeth when asked to do so, and there was no deviation of the angles of the mouth when she laughed. There was no cardiac disease. The patient's face was full of expression, and her eyes beaming with intelligence; yet it was manifest that these appearances were deceptive, and that her intellect was very much impaired. *She could not be made to understand at once, by words alone, what was required of her; and could not always answer correctly by gestures the questions which she was asked. Her pantomime was not so clear as that of a deaf and dumb individual, and she seemed not to be able to understand the meaning of words. They had to be spoken very slowly, and repeated several times before she could catch their meaning, and she most frequently failed completely in this. Gestures she understood at once. Thus, when I asked her to show me her tongue, she did not always do so immediately; but on my putting out my own tongue, and then making signs to her to do the same, she instantly complied.* She was prone to shed tears or to laugh immoderately for the least thing, as ordinary hemiplegics are well known to do, at a certain period of their complaint. She could not write a single word with her left hand: she held her pen properly, but only made a meaningless scrawl. Although she kept constantly repeating *Sapon, Sapon,* I could never make her say *Sap* or *pon* by itself, or repeat any syllable or word after me. She knew her own name, and when I mentioned it she laughed and pointed to herself. According to her sister's statement she remembered localities and knew faces well. A month after she came under observation, she had further acute cerebral symptoms, which increased her paralysis and also still further clouded her intellect for a time. But by slow degrees, and after an interval of many months, she improved remarkably, so that by the following October she was considerably better in many respects. Dr. Bazire continues:—Her intellect was improved, but not in the same proportion as the paralysis. Her emotional excitability is much less than before, although it is still marked. Her vocabulary comprises now a few more words. She still says *Sapon, Sapon,* but she can now distinctly articulate *yes* and *no,* although she does not always use them appropriately, and she can count *one, two, three, four.* When under the influence of great excitement she sometimes exclaims, "Oh, dear me," according to her sister's account. She cannot yet write a single word, nor even form a single letter, although she has often tried hard. She does not know the letters of the alphabet, and when she is shown *a* and *o,* and asked to point out *a,* she cannot do it. She has still great difficulty in understanding what is said to her in words, although she is not in the least hard of hearing; but she immediately understands gestures. Her own pantomime still lacks clearness. She never reads, but is fond of looking at pictures. The persons about her notice great fluctuations in her intellect from time to time. Her general health appears to be excellent, and she has gained flesh.

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[1] See also a case alluded to by Dr. Robertson, in 'Journ. of Ment. Sc.,' Vol. xii, p. 513.

c. I now come to the *typical* cases of Aphasia, in which category are included persons who can think, but who cannot speak or write;[1] and to illustrate this condition I will give an abstract of a case that has been related in detail by Trousseau, and which admirably illustrates most of the characteristics of the cases which would be included under this group.

M. X—, æt. 57. Very wealthy, and has lived freely without committing excesses. Became gouty at the age of forty-five, and at intervals of a year or two after this had two slight attacks of vertigo, with dimness of sight and weakness of the right hand for a time. About two years after this he had a third attack of vertigo whilst sitting in a chair. The giddiness lasted somewhat longer than on the former occasions, and from that day the right arm became more markedly weak and the tongue slightly embarrassed. This impediment attracted notice whenever M. X— spoke with animation. About three years afterwards he had a severe attack one evening. Whilst rising from his chair to shake hands with the curate of the place, he suddenly staggered, stammered, and dropped into the arms of his visitor, who had rushed forward to support him. He remained in the most profound apoplectic stupor for more than ten hours with complete paralysis of the right side. For a few days he gave only very obscure signs of intelligence; but from the time of that seizure he entirely lost the faculty of speech. A few months afterwards he almost completely recovered the power of moving his right leg, but the movements of his right arm have

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[1] See page 217. It should, in fact, be stated here that the cases belonging to groups *a* and *b,* which I have just considered, are scarcely entitled to be admitted into this first division, since their general powers of mind and of *thinking* are so much diminished. But the method I have adopted will conduce best, I think, to a clear understanding of the subject, and will show how these *typical* aphasic cases of group *c* gradually merge into others less typical, and with either less or considerably more general mental impairment than they exhibit.

have always been impeded. About four or five months after his attack (summer of 1860) he had for the first time an epileptiform seizure; he had three of these during that year, six in the following year, and four in 1862 up to the month of August; since which date he has had no return of them.

In the spring of 1863, M. X— was seen by Trousseau, who gave the following account of him. "His face was intelligent, cheerful, and full of benevolence. He seemed by his gestures, and especially by the expression of his face, pleased to see me. He could not speak, and only uttered in a faltering voice unintelligible words, in which the monosyllable '*Yes!*' returned frequently. When I questioned him he answered '*Yes!*' to everything, even when he shook his head in denial. 'How old are you?'—'*Yes!*' 'How far back do you date your illness?'—'*Yes!*' &c., &c. It could be easily seen, however, that he was not satisfied when the word 'Yes' was wrongly applied, for he then made an impatient gesture. He looked pleased on the contrary when the word was used appropriately. He sat to table with us at dinner, used his left hand, and ate with great propriety, and with infinitely greater reserve than the generality of ordinary paralytics, who often eat voraciously and in a dirty manner. He looked after his guests during dinner, and took part in some of the discussions carried on. When the delicate character of the lamb of the country was praised, he nodded assent; whilst on some of the guests saying that the kid of the country had a better flavour than the lamb, he shook his head in disapproval. He made signs to the servant to hand the wine round, and when wine of an esteemed vintage was going round he made signs that it should be drunk in preference to the rest."

"After dinner," Trousseau says, "I tried to make out how far he could give proof of intelligence. As he always answered '*Yes*,' I asked him whether he knew how that word was spelt, and on his nodding assent I took up a large quarto volume, with the following title on its back: 'History of the Two Americas,' and requested him to point out the letters in those words which formed the word '*Yes*.' Although the letters were more than one-third of an inch in size, he could not succeed in doing as I wished. By telling him to seek for each letter in turn, and by calling out its name, he managed after some hesitation to point out the two first, and was very long in finding the third. I then asked him to point out the same letters again, without my calling them out first, but after looking at the book attentively for some time, he threw it away with a look of annoyance, which showed that he felt his inability to do as I wished him."

He played every day at "all-fours," hiding his cards behind a pile of books, and using his left hand. He often won when playing with the curate, the doctor, or his son, without their allowing him to do so out of kindness. His son and Dr. Lafitte declared to me that he played as well as he ever used to do. Sometimes his son sits by his side to advise him, and stops him when he takes a card which seems not to be the proper one, but he insists on playing as he likes, and by winning the game proves to his adviser that if he sacrificed a card it was because he could thus improve his game. Although his son manages all his affairs, he insists on being consulted about his leases, contracts, &c.; and his son stated to me that his father indicates perfectly well by gestures which are understood by those habitually around him, when certain portions of the deeds do not please him, and that he is not satisfied till alterations are made, which are, as a rule, useful and reasonable.

Although his sight was good he could not read, or at least understand the sense of what he read; he listened with pleasure, however, when he was read to. He could not put together loose letters of the alphabet, nor write with his left hand. It has often happened to him to say a word which he has not uttered for a very long time, as if an old impression were revived in his brain. Some time ago he dropped his handkerchief, and as a lady near him picked it up and gave it to him, he said to her, "*Thanks!*" in a loud and distinct voice. His friends were delighted at this, and thought that he had recovered his speech. He was asked, implored, to say the word again; it was repeated to him several times, but all was in vain, he never could succeed: and this was the general rule, he could not even repeat the simplest sound which had been uttered before him. He told his age correctly in a most remarkable way with his fingers. Whenever he grew impatient, he did not always show it in exactly the same way. Before strangers he contented himself with a very significant movement of the shoulder, shrugging it with a look of discouragement and *ennui*. But when he was alone with his wife, his son, or his servants, he showed his impatience by using a very well known oath.

This case may be considered a very typical one, and illustrates the condition of patients who by no means unfrequently come under our notice. Sometimes, as in a case which I have at present under observation, the patient is able to read so as thoroughly to understand a book or a newspaper. My patient is also able to write a few simple words with his left hand. Trousseau's patient, however, was unable to do either of these things.

d. The slighter or *less severe* cases of aphasia are connected by others, of all intermediate degrees of severity, with those to which I have last referred. In the slighter cases the individual can speak more words, he can imitate and repeat sounds to a certain extent, and he can express himself to a limited extent in writing. Such cases are often not associated with hemiplegic symptoms, as may be seen by the example which I also extract from Trousseau's work, though in other instances well-marked hemiplegia may exist.[1]

A young labourer, æt. 28, had, according to the statement of his friends, been seized suddenly and without any assignable cause with complete mutism. His previous health had been good on the whole; he had led a regular life, but two years previously he had suffered from violent headache, and had even been delirious. For this he was bled from the arm, and got well again; and since then he had not been taken ill in the same way.

The affection for which he came to the Hospital consisted solely in *an utter inability to speak, although his intelligence seemed to be unimpaired,* and he could perfectly understand all the questions that were put to him. But to these questions he invariably answered "*No,*" even when he nodded his head to signify assent. One of the students, however, informed me that when left alone with him, he had succeeded in making him say the word "*cloak,*" after many repeated trials. I found only a marked deviation of the apex of the tongue to the right, but no other sign of paralysis; the face, the trunk, and limbs, could be moved with perfect freedom and force. The second day after the patient's admission I ordered him to be bled, and after this he seemed to be able to move his tongue more freely than before, but there was the same complete aphasia. When I asked him to write his name down he did so correctly, but when I told him to write down what had happened to him, he only wrote "*was, was, was.*" He knew perfectly well that this was not what he wanted to write, and, annoyed at not being able to express his thoughts, he put down the pen. Two days after this, on my asking him to write down the name of his birth-place, he wrote "*alone, alone, alone,*" and did so again when I asked him to write "good morning." His impatient gestures, all the while, showed that he was perfectly conscious that he was not writing what he had in his mind.

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[1] 'Translation,' by Dr. Bazire, p. 224.

On the following day he wrote again words that had no sense, such as "*game*" for "*soup*," but he could say, "*Good morning, Sir,*" speaking, it is true, like a child who is learning to speak. A few days later he said very distinctly: "*I am pretty well,*" and then "*Good morning, Sir, I am getting on well,*" with a hesitating voice, however, like an habitual stammerer, who endeavours not to stutter. When the attempt was renewed to make him write, he only scribbled on the paper series of syllables without any meaning, but he managed to write under dictation: "*I have eaten.*" He left the Hospital about a fortnight after his admission, though there had been no appreciable change in his symptoms. He himself asked for his discharge, however, saying very distinctly: "*I wish to go away!*"

It is a matter worthy of note, that, in this case, the inability to write showed itself in great part as an amnesic defect.

II.—APHEMIA.

I now pass on to the consideration of a very remarkable class of cases—the examples being very scarce—in which the individuals, whilst incapable of speaking, are nevertheless able to express themselves perfectly in writing. This is the class for the designation of which we should reserve the term Aphemia. Trousseau, after stating his belief that in the forms of Aphasia which I have just considered, there is always some amount of mental impairment, however slight, says: "There is, however, a form of aphasia in which the intellect is unaltered. Memory is good, the patient writes easily, and expresses his thoughts correctly in writing, as educated deaf-mutes do. This form is very rare, and I have thought myself warranted in regarding it as a distinct variety, particularly as in all the cases of the other form of the disease, the inability to write is proportionate[1] to the inability to speak." He then gives some details concerning one of these cases—that which made the most impression upon him. He says:—

I received one day in my consulting-room a carrier of the Paris Halles, very young, and having the appearance of a man enjoying excellent health. He made signs that he could not speak, and handed to me a note in which the history of his illness was detailed. *He had written the note himself, with a very steady hand, and had worded it well.* A few days previously he had suddenly lost his senses, and had been unconscious for nearly an hour. When he came round he exhibited no symptom of paralysis, but *could not articulate a single word.* He moved his tongue perfectly, he swallowed with ease, but, however much he tried, he could not utter a word. He was ineffectually galvanized for a fortnight, but without any special treatment he completely recovered his speech five or six weeks after the invasion of the complaint. It is very remarkable, however, that *during the whole course of this singular affection he could manage all his affairs by substituting writing for speech.*

In this case the man could not articulate at all, and therefore was unable to read aloud in any fashion, although we may fairly presume that he was able to comprehend what he read silently; but in another very remarkable case, which was most carefully investigated and recorded by Dr. Osborn,[1] the patient was able to talk a meaningless jargon; and when he attempted to read aloud gave utterance also to a series of articulate sounds, having no intelligible meaning or resemblance to those which he should have uttered. I subjoin some of the principal particulars concerning this case.

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[1] I do not think this latter statement of Trousseau is altogether correct.

A scholar of Trinity College, Dublin, twenty-six years of age, of very considerable literary attainments, and well versed in French, Italian, and German, after having bathed in a neighbouring lake, and whilst sitting at breakfast, suddenly had an apoplectic fit. He was reported to have become "sensible in about a fortnight," but, although restored to the use of his intellect, he had the mortification of finding himself deprived of speech. He spoke, but what he said was quite unintelligible, although he laboured under no paralytic affection, and uttered a variety of syllables with the greatest apparent ease. When he came to Dublin his extraordinary jargon led to his being treated as a foreigner in the hotel where he stopped; and when he went to the College to see a friend he was unable to express his wish to the gate-porter, and succeeded only by pointing to the apartments which his friend had occupied.

Dr. Osborn, after frequent careful investigations, ascertained the following particulars concerning his patient:—

1. He perfectly comprehended every word said to him.

2. He perfectly comprehended printed language. He continued to read a newspaper every day; and when examined proved that he had a very clear recollection of all that he read. Having procured a copy of Andral's 'Pathology' in French, he read it with great diligence, having lately intended to embrace the medical profession.

3. He expressed his ideas in writing with considerable fluency; and when he failed it appeared to arise merely from confusion, and not from inability, the words being orthographically correct, but sometimes not in their proper places.

4. His general mental power seemed unimpaired. He wrote correctly answers to historical questions; he translated Latin sentences accurately; he added and subtracted numbers of different denominations with uncommon readiness; he also played well at the game of draughts.

5. His power of repeating words after another person was almost confined to certain monosyllables; and in repeating the letters of the alphabet he could never pronounce *k*, *q*, *u*, *v*, *w*, *x*, and *z*, although he often uttered these sounds in attempting to pronounce the other letters. The letter *i*, also, he was very seldom able to pronounce.

6. In order to ascertain and place on record the peculiar imperfection of language which he exhibited, Dr. Osborn selected and laid before him the following sentence from the bye-laws of the College of Physicians, viz. "*It shall be in the power of the College to examine or not examine any Licentiate previous to his admission to a Fellowship, as they shall think fit.*"

Having set him to read, he read as follows:—"*An the be what in the temother of the trothotodoo to majorum or that emidrate eni enikrastrai mestreit to ketra totombreidei to ra fromtreido as that kekritest.*" The same passage was presented to him in a few days afterwards, and he then read it as follows:—"*Be mather be in the kondreit of the compestret to samtreis amtreit emtreido and temtreido mestreiterso to his eftreido tum bried rederiso of deid daf drit des trest.*"

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[1] 'Dublin Journ. of Med. and Chemical Science,' Vol. iv. p. 157.

Dr. Osborn observes, that there are several syllables in the above of frequent occurrence in the German language, which probably had made a strong impression upon the patient's memory. He appeared generally to know that he spoke incorrectly, although he was quite unable to remedy the defect. After the expiration of eight months, however, he was so far improved that he was able to repeat the same bye-law after Dr. Osborn as follows:—"*It may be in the power of the College to evhavine or not ariatin any licentiate seviously to his amission to a spolowship, as they shall think fit.*" Some little time after this Dr. Osborn says he "repeated the same bye-law after me perfectly well, with the exception of the word 'power,' which he constantly pronounced *prier*. He was also able to pronounce all the letters of the alphabet except *d*, *k*, and *c*." He progressed in this way under the directions of Dr. Osborn, who advised him to commence learning to speak again, like a child repeating first the *letters* of the alphabet, and subsequently words, after another person, on the ground that he had "lost, not the power, but the art of using the vocal organs."

Another most remarkable case has been recorded by Bouillaud,[1] presenting many features in common with that just narrated. But in this case the man did not generally speak in an unintelligible jargon; he mostly made use of actual words, though of such a kind, and so collocated, as to have no resemblance to what he ought to have said. In reading aloud, however, he did often utter mere jargon.

Lefevre, æt. 54, after some extreme mental anxiety, became unable to read, or find words to express his thoughts. His sensibility and powers of movement were unimpaired, and his general health was pretty good. When he wished to reply to questions that were addressed to him, he made use of expressions either quite unintelligible, or else having a meaning quite different from that which he intended to convey. When questioned as to his health, he replied rightly in two or three words; then in order to say that he did not suffer at all from pain in the head, he said, "*Les douleurs ordonnent un avantage,*" whilst in writing he replied to the same question in this way:—"*Je ne souffre pas de la tête.*" When a word such as "*tambour*" was pronounced, and he was asked to repeat it, he said "*fromage,*" though he wrote it, on the contrary, quite correctly when asked to do so. He was requested to copy the words "feuille médicale;" he wrote it perfectly, but could never exactly read the word which he had just written; he pronounced instead, *oquicale*, *fénicale* and *fédocale*. Then when made to read the word *féquical*, written by himself, he pronounced it *jardait*. He often wrote upon paper phrases which were unintelligible, either by the nature of the words that he employed, or by their lack of relation to one another. When he was shown different objects, he generally named them correctly; but then he was wrong at times, and during the same sitting he called "*une plume, un drap; un crachoir, une plume; une main, une tasse; une bague, un crachoir.*"

These three cases have many points in common—in each there was no paralysis of the hemiplegic kind, and the mental faculties in the first two, at least seemed quite unimpaired: whilst there was no power of articulating at all in the first, there was a choreic and utterly unintelligible jargon-like speech in the second, and in the third individual there was a condition, in which actual words were pronounced, though these—on account of their inappropriateness and want of correlation with one another—were such as we might imagine would be made use of by an individual in whom the incoordinate condition of memory for words (*Amnesia*) existed in its most extreme form. Yet this last individual, when he read aloud. uttered a mere jargon as in our first case—though, notwithstanding these defects in speaking and reading, he was able to write what he wished quite correctly.[1]

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[1] 'Traité de l'Encéphalite,' and quoted by Marcé in 'Mém. de la Soc. de Biologie,' 1856, p. 100.

III.—AGRAPHIA.

The cases included under this head—which are also very rare—are, in a measure, the converse of those last described, so far as the powers of expression are concerned; the individuals in the former cases could write, but could not speak; these can speak but cannot write proportionately well. Such individuals should therefore be called *Agraphic*, rather than *Aphemic*, and they differ from those who are more properly named *Aphasic* in respect of the essential fact that they are deprived of only one of the modes of expression, since they are still able to speak. The cases illustrative of this defect are not quite so well defined and characteristic as those I was able to quote in elucidation of the last. The most striking[2] case I have met with has been recorded by Dr. Hughlings Jackson, who has laboured so indefatigably to advance our knowledge on the whole question of the loss of speech in cerebral disease. I give this case[1] nearly in his own words.

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[1] In connection with the cases just narrated I will give another quoted by Dr. Winslow, (loc. cit., p. 328) where there was also greater power of writing than of speaking, though the power of writing was much diminished by reason of the incoordinate condition of memory for words manifesting itself during the exercise of this function, as it did in speech in the third of the cases above related.

"A gentleman, forty-six years of age, who had always enjoyed a good state of health, after experiencing great uneasiness of mind, and being exposed to severe bodily fatigue, was seized with apoplexy followed by hemiplegia. The apoplexy was slight, but the hemiplegia was complete. The power of speech was entirely lost, so that he could only utter the sounds *ee-o*, which, however, he so varied that, with the assistance of expressive gestures, he was able to convey to those about him his meaning, very distinctly, upon ordinary subjects. He perfectly comprehended everything that was said to him, and clearly understood what he meant to answer, but was able only to utter the previously mentioned sounds. *Believing, however, that he actually employed the words adapted to the communication of his ideas, he often appeared surprised and displeased when he was not understood.* He sometimes endeavoured to explain his meaning by writing on a slate; but he generally substituted one word for another, and almost always erred in spelling what he wrote."

Dr. William Ogle also records a case (*loc. cit.*, p. 105), in which the power of writing was much more extensive than the power of speaking.

[2] I say the most striking, because in this case the contrast was the greatest

A woman, sixty-nine years of age, was admitted in May, 1864, to the National Hospital for the Paralysed and Epileptic, under the care of Dr. Ramskill, and by him transferred to Dr. Jackson's care.

She was healthy-looking, and, except that she had had rheumatic fever fifteen years before, had enjoyed good health. Five weeks before admission she suddenly became ill, and lost the entire power of speech for a week, and was also paralysed on the right side. When seen there was no apparent hemiplegia, but she complained of weakness in the right side. I could hear no bruit, but the heart's action was very irregular. When I saw her she could talk, but made mistakes. For instance, when I was trying her sense of smell, which was very defective since the paralysis, she said in answer to a question, "I can't *say* it so much," meaning she could not smell so well. She frequently made mistakes in speaking, and called her children by wrong names. *This was never very evident when she came to the hospital, and might easily have been overlooked,* but her friends complained much of it. She seemed to be very intelligent. Her power of expression by writing, however, was very bad, although her penmanship was pretty good, considering that she wrote with the weakened right hand. She wrote the following at the hospital. I first asked her to write her name—I do not like, for obvious reasons, to give her real name for comparison—it had not, however, the slightest resemblance to the following, in sound or spelling,—

"SUNNIL SICLAA SATRENI."

When I asked her to write her address, she wrote :—

"SUNESR NUT TS MER TINN— LAIN."

Thinking she might have been nervous when she wrote at the hospital, Dr. Jackson asked her to bring something that she had written at home. She did so, but the specimen, the fac-simile of which is given, was not in the least better than what she had previously written. It is a perfectly meaningless assemblage of letters, notable only for the frequent repetition of certain of them.

Now it is clear that in this case the defects of speech, which were of the *Amnesic* kind, were utterly disproportionate to the defective power of writing—all capability of expression in this way was in fact abolished, although the power of shaping individual letters remained. In another case recorded by Dr. William Ogle[2], an ataxic inability to write was associated with a graver amnesic condition as regards speech.

James Simmonds, fifty-four years of age, after a heavy blow on the left side of the head, seven years ago, was obliged to give up his work. He spoke without difficulty or hesitation, but miscalled things strangely. He then had a fit one morning, whilst dressing, which left him speechless and hemiplegic on the right side. For a fortnight he could not speak at all, though he was quite sen-

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between the power of speaking and the power of writing. The defect in writing was, however, one of an amnesic kind, though in the next case it was most strikingly ataxic.

[1] 'Lond. Hosp. Repts.,' vol. i, p. 432.

[2] Loc. cit., p. 103.

> sible. He could not say so much as "yes" and "no." From this he gradually recovered, but always, as before, miscalled things. He frequently called his children by the wrong name—would say "Peter" for "James," for instance. A month ago he had a second fit, which left him with less power than before in his right side, but made little or no change in his speech. When this second fit occurred he was suffering, as he had often done, from a slight attack of gout in the hand.
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> There is now partial paralysis of the right side, which does not prevent his walking. The facial muscles on that side are slightly affected as well as the limbs. His speech is very hesitating and imperfect. He often stops suddenly, at a loss for a word, and then frequently uses a wrong one. As, for example, he substituted "barber" for "doctor," "two shilling piece" for "spectacles," "winkles" for "watercresses," &c. *He can, however, pronounce perfectly any word when he is prompted.* He says that he generally knows when he has used a wrong word, but not always.
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> Before his illness he wrote a good hand, and was above his lot as regards education. *Now he cannot form a single letter.* Even with a copy before him he makes only uncertain up and down strokes. I gave him some printed letters, and asked him to pick out his name. After a long time he arranged JICMNOS. Clearly he had some slight notion of the letters which composed his name. According to his wife, before his illness, he spelt well, and was very particular about the spelling of his own name, which is one admitting of many variations. When a copy was before him he quickly picked out and arranged the name correctly. *He can read;* but he says that reading makes him very giddy, and causes great pain in the head. His general understanding seems good, and up to the average of men in his class.

Dr. Jackson has also recorded another case[1] in which the power of writing and spelling was very much impaired, whilst that of speech was affected only to a more trifling extent. This man had "performed the duties of an important government office, requiring good education and intelligence," and he had been subject to a series of epileptiform attacks, at first principally involving the left side of the body, but then, after an interval, affecting the right side instead. The defects in the power of expression which are about to be noted occurred only after the second series of fits. Dr. Jackson says, "After these attacks, the patient could talk, but he made mistakes in talking. There was, however, so much incoherence with this particular defect, that I did not then come to any positive conclusion as to how far this bad talking was to be classed in the same category with what has been described under the names of aphemia and aphasia." A few weeks after this Dr. Jackson met this patient in the street, and says, "He was then, to superficial appearance, as well as ever. *I observed that he spoke quite well, and this throughout rather a long conversation.*" The patient said, however, that he was often at a loss for a word; and his father told me that his son frequently made mistakes in names. His greatest trouble was in writing—he had no difficulty about the mere penmanship, this was excellent. "His trouble was that he could not readily find the proper words, and those he wrote he often spelled incorrectly. He said he could generally manage to write a pretty good letter if he made a copy first and then corrected this—in an example given, however, several words were still spelled wrongly. The following is a copy of one of his letters, in which the words in square brackets are corrections written by the patient, whilst those in the curved brackets are explanatory by Dr. Jackson himself.

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[1] 'Brit. Med. Journ.,' 1866.

> "I do not find any improm (crossed out, and 'improvement' substituted) in by leth [my health] this dalt [last] few days. Bodily I seem about the same, with the exempt—except that I ave, av have had indejention [indigestion]. I can recofect things for years past very well, but I am pusseld [puzzled] to find how to spell, and cannot do so with (without?) thinding [thinking] first. I seem to av have got got on better this (crossed out) with this better (letter?) than I some thim (crossed out) times can."

He was able to copy a paragraph from a printed book well, making only one or two trivial errors; but in attempting to write from dictation, he made very much worse mistakes in spelling than occur in the letter above cited. When asked to spell words, he also succeeded very badly; and though he repeated perfectly even the most difficult sentences, when he attempted to read aloud he could not succeed at all, pronouncing almost every word of two or more syllables wrongly.[1]

Of these three cases it will be seen that the agraphic symptoms in the first two occurred in individuals who were also hemiplegic on the right side, whilst the third had been subject to epileptiform convulsions affecting the right half of the body. I will now give some account of an individual in the Broadmoor Lunatic Asylum who presents most remarkable agraphic symptoms of an amnesic variety, but who suffers neither from paralysis nor from epileptiform convulsions of any kind.

The man was formerly a sailor, is now about 45 years of age, and partially demented; he was formerly violent and dangerous, but without obvious delusions. He was certified to be insane in 1855, though, as just stated, his insanity did not manifest itself at all in the form of delusions. It was not till about the year 1857, or later, that he began to write in an extraordinary manner; previously to this his letters to his friends are stated to have been written in a perfectly intelligible manner. The peculiarity manifested itself first in this way: he commenced the writing of each word correctly, and then in the place of some of the remaining letters he wrote *ffg*. Afterwards, the whole character of the word became altered, and duplication of many of the consonants, together with an almost invariable termination with the letters *ndendd*, or, at least, *endd*, became the most noteworthy features of writing which though produced voluminously was almost utterly unintelligible. When I was in the habit of seeing him, about three years ago, he gave me pages and pages of his peculiar writing at different times, and from what I have in my possession I have cut out the sixteen specimens which have been lithographed. These show, at a glance, that he either wrote with a peculiar and continual iteration of certain sets of letters, the writing being partly intelligible, or else that it was a mere succession of letters or strokes to which no meaning whatever could be attached. The principal peculiarities about this case are, that whilst the patient writes in this fashion he speaks in the ordinary way, so far as he can, considering his demented condition and somewhat incoherent state of mind. Fearing lest my memory might have failed me concerning certain points, I wrote to my friend, Dr. William Orange, to let me know what his condition is at present. He very kindly submitted him to a careful examination, and his replies are quite in accordance with my own recollections of the case, though it seems that he has now become more demented, whilst his special defect is much less marked than it was. The principal peculiarities observed were as follows:—

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[1] Here again, as in the case recorded by Dr. Hun (p. 220), the ability to read aloud was commensurate rather with the power of writing than with that of speaking. Both reading aloud and writing require the integrity of the 'visual perceptive centre,' and that this was more impaired than the 'auditory perceptive centre' in this latter case, seems clearly indicated by the facts recorded above, that he could repeat even the most difficult sentences correctly—an operation in which the auditory perceptive centres were called into play, but not the visual—whilst he could not read aloud the simplest passage without making many mistakes. In the next case, however, the manner of reading aloud does not agree with the manner of writing, but is about on a par with the power of speaking.

1. He can speak fairly well for a short time, but his attention wanders, and his voice then becomes drawling and monotonous, when he often either mispronounces a word (generally by altering its termination) or he substitutes another word or mere sound having no meaning.

2. He can read the newspaper either to himself or aloud—but he does not seem to gather the full meaning without effort, and his power of continuous effort is small. When he reads aloud he stumbles over the difficult words, and he reads in a drawling tone, but the words which he utters, if not actually those before his eyes are words of a somewhat similar sound, and do not appear to have any obvious relation to the peculiar style of his writing.

3. He spells a word, when asked to do so, in the way in which he would write it, and then he pronounces it correctly immediately afterwards.

4. He can play at cards or draughts, but he seldom does play, according to his own account, because of the quarrelling; but there is no doubt the chief reason is apathy, and the mental effort which playing causes him. His memory is much impaired, especially as to recent events.

With regard to the specimens of his writing which are given in the accompanying Lithographic Plate, No. 1 is a portion of the longest piece that was ever intelligible to me; it seemed intended for "Royal naval medical officer belonging to Admiralty." Comparison of this with the second and third specimens (written at different times), will show, in the first place, that he did frequently write the first two or three letters of words correctly—although at other times even these were quite wrong or doubled—whilst the remaining portion of the word was replaced by *endendd* or *ndendd*; and secondly, that he spelled the same words in a similar way at different times. Having been a sailor in the navy, the words, "royal," "naval," and "doctor," appeared frequently in his writings, and these were almost invariably written in the same way. But this was not always so with other words which he did not employ so frequently; thus, the first word of No. 4 seems also intended for my name, though he usually wrote it as given in No. 3. Here it seems that the fact of the repetition of the letter *a* in the name appears to have had its influence in changing his termination from *dendd* to *dandd*. No. 6 is an example of some of the worst of his writing, in this style, in which there is no attempt at the formation of words, but the mere continuous stringing together of letters. Specimens 7, 8, and 9, are examples of a style of writing which he adopted for a few weeks, and from the first "Belonging to Royal," it will be seen that *nghengg* has replaced the old termination. Whilst he wrote in this way and in that first described he constantly made use of two words spelled rightly, *of* and *to*, and in No. 5 two consecutive words are spelled properly, though in all his writings I have never seen this occur elsewhere. When writing in the way first described he invariably wrote "*themedd*" instead of "the," and just as invariably when writing in the second manner he (No. 8) spelled it "*theme*" simply. No. 10 is a form of writing of which I have only seen one specimen. The remaining kinds (11—16), though I have arranged them in an order of increasing simplicity, were by no means written at successive times, in the same order. He would adopt one of these forms for a time—perhaps for two or three weeks—and then generally return for a much longer period to his *ndendd* style, subsequently relinquishing this again, and adopting some other form. For some considerable time he wrote after the fashion shown in No. 12, when *entens* and *entent* perpetually recurred. I have seen whole sheets of foolscap closely covered with markings in each of the styles 13, 14, 15, and 16; and at the present time he is writing, as shown in No. 11, which was selected from a specimen forwarded to me by Dr. Orange. The first two modes are the only kinds of writing in which words were at all recognisable, and this generally in the first few lines only; after these his manuscripts became almost entirely unintelligible.

It seems to me most interesting to compare this case with that recorded by Dr. Osborn; there the patient could write properly, though he spoke in a mere jargon, in which the same sounds frequently recurred; here our patient (barring his demented condition) speaks properly, though he writes a mere jargon of letters, with a constant repetition of certain of them.

(*To be continued.*)

PART FOURTH.

Chronicle of Medical Science.

(CHIEFLY FOREIGN AND CONTEMPORARY.)

CHRONICLE OF PHYSIOLOGY.

By Henry Power, F.R.C.S., M.B. Lond.,
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Part I.—Blood-Circulation.

1. Prof. E. Neumann: *On the Influence of the Marrow of Bones in the formation of Blood-corpuscles in Centralblatt* 1868, p. 689.
2. H. Jacobson and M. Bernhardt. *On the Difference of Temperature between the right and left sides of the Heart.* Idem., p. 643.
3. Friedländer. In Untersuch aus der Physiolog. Laboratorium zu Wurzburg. A. v. Bezold.

1. This is a preliminary notice by Professor Neumann, in which he states that the so-called red marrow of the bones in man as well as in rabbits, contains beside the well-known cells of the medulla, certain other elements which have not been described, namely, nucleated red blood-corpuscles, agreeing in all respects with the corpuscles which appear in the first stages of the development of the fœtus. Similar corpuscles, although fewer in number, are found in the ordinary fatty marrow, their number and that of the cells of the medulla diminishing in proportion as the fat cells increase in number. He refers the origin of the nucleated blood corpuscles to the medulla cells, since he has observed a large number of white corpuscles in the blood of the medullary vessels, rendering it highly probable that a continual immigration of medullary cells into the vessels takes place.

2. The means of estimating the difference of temperature employed by MM. Jacobson and Bernhardt, was a thermo-electric apparatus, each division on the scale of which corresponded to 0·062° Cent. The wires were thrust into the heart of the living rabbit, either through the skin, or after the division of this through the muscles, and by practice they were able at length to place the point of junction of the two metals in the interior of each ventricle. The result obtained was, that only in two cases was the blood in the two ventricles of equal temperature. The left in all the rest was

warmer than the right to the extent of from 0·12° to 0·42° Cent. This is in opposition to the results obtained by G. v. Liebig and C. Bernard, who employed very delicate mercurial thermometers, but there can be no doubt that the insertion of a thermometric wire must produce much less disturbance to the circulation than the passage of even a delicate thermometer through the jugulars into the ventricles. No coagulation was observed around the wires, even when the observation was prolonged to the extent of half an hour, the relative temperature remaining the same throughout, and the heart maintaining its normal rhythm. When by mistake the wire entered the pericardial or pleural cavity, these were always found to be from 0·2° to 0·5° C. colder than the left heart; but, again, in opposition to the views of Bernard, rather higher, 0·06° to 0·1° C. higher than the liver, into which also by error it sometimes penetrated.

3. In this paper M. Friedländer first enters into a consideration of the central nervous organs of the heart of the frog. He agrees with Budge in believing that the only nervous cords supplying the heart in this animal proceed from the vagus. The cardiac branches of the vagi pass to the venæ cavæ and pulmonary veins, and form ganglionic plexuses on the walls of the auricles and ventricles. He calls in question the statement of Kölliker that the greater number of the ganglionic nerve-cells have only one nerve arising from them, since though they are not uncommonly unipolar, they present the characters of the ganglion cells described by Beale and Arnold, each cell giving origin to two fibres, one of which winds spirally for some distance round the other, or occasionally runs parallel to it. In the second part of his essay, M. Friedländer gives some interesting observations on the automatic movements of the heart in the frog. In these experiments he snipped off minute portions sometimes not exceeding 1-125th of an inch in diameter of the muscular tissue of the heart, and immersed them in an artificial serum formed by adding 0·8 or 0·9 parts of common salt to 100 of water, and to this about 10 parts of albumen of egg. The portions of muscle retain their contractility in such fluid for no less a period than twenty-four or thirty-six hours, though sometimes only two or three muscular fibres were present. The question naturally arose, did these minute fragments of muscle beat automatically and independent of all action of the nervous system, or were ganglionic nerve-cells present? Upon careful investigation, M. Friedländer states that he was invariably able to prove the presence of nerve-cells scattered amongst the fibres, and he, therefore, adopts the view now generally held that the contractions of the heart are brought about by the direct agency of the nervous system; at any rate, these experiments show that the presence of blood is not requisite to maintain the rhythmical contraction of the heart in these small fragments. M. Friedländer finds that as M. Cyon had previously stated in respect to the whole organ, elevation of temperature occasioned increase of energy in the heart's action, or renewal of the movements if they had already ceased.

PART II.—NERVOUS SYSTEM.

1. Schultze: *Observations on the Structure of Nerve-cells and of Nerve-fibres.* (Bonn, 1868.)
2. Heidenhain: *On the reaction of Active Nerves.* (In the Studien des Physiolog. Instituts zu Breslau, Heft. 4, 1868, p. 248.)
3. Liebreich: *On the reaction of Living Nerves.* (In the Tageblatt der Naturf. Vers. zu Frankfurt, 1867, p. 73.)
4. Ranke: *On the Chemical Reaction of Nerves.* (In his essay on the Vital Conditions of Nerves. Leipzig, 1868.)
5. Kowalewsky and Adamük: *On the Depressor Nerve of the Heart.* (In Centralblatt f. die Med. Wiss., 1868, p. 545.)
6. H. Aubert and G. Roever: *On the Influence of the Nervous Vagus, the Superior Laryngeal and Sympathetic Nerves on the Pressure of the Blood and on the Frequency of the Pulse.* (Idem. p. 577.)
7. Fr. Goltz: *Essay on the Reflex Inhibitory Influence that can be exerted on the Heart.* (Idem., p. 593.) Also, *On the Functions of the Brain in Frogs.* (Idem., p. 690).)
8. M. J. L. Prévost: *Anatomical and Physiological Investigations on the Spheno-palatina Ganglion.* (In the Archives of Physiologie de M.M. Brown-Séquard, Charcot, and Vulpian, T. 1, 1868, p. 7.
9. Ch. Rouget: *Memoir on the Nervous Corpuscles which are found at the origin of the Sensory Nerves in the Papillæ of the Skin and of the Mucous Membranes.* (In Idem., p. 591.)
10. A. Rauber: *Researches on the Pacinian Bodies* (Munich, 1867.)
11. M. J. S. Lombard: *Experiments on the Influence of Intellectual Work on the Temperature of the Head.* (Idem., p. 670.)

1. The following is a translation of an abstract of M. Schultze's essay, by M. Busch, contained in the 'Centralblatt' for October, 1868. Schultze's observations were chiefly made on the nervous centres of the torpedo, though the results are also applicable to those of mammals. He finds that the multipolar *cells* of the spinal cord of the electrical organ and of the cortex of the brain, consist for the most part of extremely delicate fibres with a finely granular matrix. These fibres cross one another in all directions and at certain intervals unite to form bundles which constitute the caudate prolongations or processes of the cell. No definite arrangement of the fibres could be discerned, nor could one of the fibres be traced extending between two processes. Thus, the cell processes of nerves are fibrillar, and this holds good not only of the unbranched axis-cylinder prolongation, but of the ramified processes. Both contain near the cell granular matter, which at some little distance from the cell gradually diminishes to a minimum in the ramified processes of old torpedos. The granular matter is often accumulated into little heaps, especially at the points of division, and stained yellow. The medullary sheath is often seen to commence abruptly on the axis cylinder at a short distance from the cell from

which it arises. No medullary sheath ever appears on the branched processes, nor do the processes of neighbouring cells ever anastomose. In the brain of the torpedo no cell ever gives origin to more than one axis-cylinder. He could never perceive either in the brain or spinal cord of the torpedo or of mammals the origination of fibres from the nucleus of the cell, and he, therefore, holds that this mode of origin as described by Arnold and Frommann is extremely rare. Isolated primitive fibrillæ of nerves are only seen in the finest central and peripheral ramifications. It is probable that the primitive fibrillæ commence in the branched ramifications of the nerve-cells; these converge to the cell, and issue in a bundle as the axis-cylinder process. The fibrillar nature of the nerve-fibres in vertebrata offers an analogy hitherto sought in vain to the structure of the nerve-fibres in invertebrata, in which the nerves are composed of extremely fine fibrillæ. Schultze's observations were made on specimens which were either quite fresh or hardened in one or other of the ordinary modes.

2. Heidenhain calls in question the acid reaction of nervous tissue after death, since he was unable to discover any discoloration of litmus paper or solution in nerves crushed either before or after tetanisation.

3. Liebreich, again, never observed an acid reaction in living nerve; whether before or after tetanisation, it was always either neutral or alkaline.

4. Ranke, on the other hand, though he found that perfectly fresh nerves were alkaline, observed that after electrization or after tetanus produced by strychnine, the reaction was invariably acid.

5. It is now well known that MM. Ludwig and Cyon have described a depressor nerve of the heart usually arising from the superior laryn-geal nerve, or from this and the pneumogastric in rabbits. MM. Kowalewsky and Adamük undertook a series of researches to ascertain the existence of a similar nerve in other classes of animals, and communicate the results of their first investigations on cats. They are as follows: 1. They have only found a depressor nerve isolated from the vagus and sympathetic in five animals, though they sought for it in more than fifty. 2. When present, however, the nerve possessed all the characters of the depressor nerve, described by Ludwig and Cyon, electrical stimulation of its cranial extremity inducing a remarkable diminution of the pressure in the arterial system, and a retardation of the beats of the heart. It is necessary, however, that at least one vagus should remain intact, in order that the retarding influence upon the heart should be exerted. Excitation of the peripheric extremity is not followed by any marked phenomena, either on the pulse or on the intra-arterial pressure. 3. The vagus in cats constantly possesses depressor fibres even where an isolated depressor exists. (This statement is confirmed by Dreschfeld also for rabbits.) The excitation of the central extremity of the vagus in animals under the influence of woorara, induces extraordinary depression of the arterial pressure, amounting to as much as 100 millimètres of mercury in the carotid.

4. It is not a *sine quâ non* that the cerebrum should be removed in order that the depressor function of the vagus should be exercised in cats, though Dreschfeld states that it is for rabbits. 5. The depressing influence of the vagus may, with a certain strength of excitation applied to the proximal extremity of that nerve, be recovered from, so that after an inconsiderable diminution of pressure, there may occur remarkable exaltation of it or a series of alternations or waves; or, finally, it may happen that no depressor influence is exerted, but a marked exaltation of pressure in the arterial system occurs at once. But however this may happen there is always a certain strength of stimulus by which the depressor influence can be clearly made apparent. If to these statements it be added that in the cat a thin nerve-trunk distinct from the vagus, depressor, and sympathetic nerves, may sometimes be discovered, the excitation of the central end of which effects strong elevation of the blood pressure, we have a certain amount of evidence that there exists either in the trunk of the vagus, or rarely isolated from it, fibres, excitation of which augments the pressure in the vessels, and which are, therefore, opposed in their action to that of the depressor nerve, so far as the pressure of the blood in the arteries is concerned. MM. Kowalewsky and Adamük have noticed an analogous exaltation of the pressure on irritation of the central extremities of a few other nerves; as, for example, the phrenics. Whether these fibres constitute a true reflectorially exciting apparatus for the vaso-motor centres or not, is a point to which their attention is now devoted.

6. The experiments of Aubert and Roever were made on the central ends of the divided vagus and sympathetic in the neck, especially in reference to MM. Ludwig and Cyons' depressor nerve, and also to the action of the superior laryngeal on the pulse and blood-pressure in rabbits, cats, dogs, and lambs, subjected to the influence of woorara. They obtained very variable results in regard to the vagus. Thus, in rabbits, the vagus usually operates as a depressor agent on the blood-pressure, but sometimes as an exalter of pressure. And in one animal stimulation of the right vagus caused an increase of pressure, whilst stimulation of the left caused a depression of more than 20 per cent. Injection of morphia caused no observable difference in the effects. In cats a diminution of the blood pressure to the extent of 50 per cent. was constantly observed, the greatest care being taken that the depressor nerve was not coincidently excited. In dogs, in which animals the vagus is only exceptionally distinct from the sympathetic, irritation of its centric extremity, in one case where it ran a separate course, and in others where it was artificially separated, produced a diminution of pressure of about 30 per cent., but in one case an elevation of as much as 100 per cent. Excitation of the conjoined vagus and sympathetic centric extremity produced without exception exaltation of pressure; after excitation the blood-pressure either fell to its previous degree or below it. In lambs, in which also the vagus and sympathetic nerves run a conjoined course, excitation either of the vagus or of the common trunk produced constantly an exaltation of blood-pressure

amounting to 60 per cent. These results, as far as regards blood-pressure, were equally observable, whether the nerves of both sides were cut through, or whether only one vagus and sympathetic or vago-sympathetic trunk were experimented on, the other remaining intact, but the results as regards the frequency of the pulse differed in these two cases. Thus, if the vagus and sympathetic of one side were intact, excitation of the central extremity of the vagus or of the vago-sympathetic nerve, produced more or less retardation of the pulse, and sometimes even stoppage in rabbits, cats, and dogs, especially in the last-named animals. But if both vagi and sympathetics were divided, no retardation of the pulse occurred, but sometimes a slight increase in the frequency of the cardiac beats. They draw the conclusion, therefore, that the excitation of the vagus on one side is transmitted through the corresponding nerve of the opposite side. Excitation of the central extremity of the cervical sympathetic, in all the above-named animals, caused constantly an exaltation of blood-pressure; no alteration in the frequency of the pulse was observed whether the vagi were preserved uninjured or even divided. Excitation of the central extremity of the superior laryngeal nerve in dogs, cats, and lambs, usually caused an increase of blood-pressure without remarkable change in the frequency of the pulse, but in exhausted animals of these genera, and in rabbits, no effects were noticed.

7. Prof. Goltz remarks that if a frog be struck repeatedly on the belly the heart stops in consequence of reflex excitation of the vagus, but this effect is not produced if coincidently with the blows the sensory nerves of the extremities are powerfully excited as by pinching, or by the shocks of an induction apparatus. If the animal be allowed to rest for a few minutes, and the blows on the belly are repeated without irritating the sensory nerves, the heart will again stop in diastole. The explanation he suggests is, that violent irritation of sensory nerves induces a condition analogous to paralysis in the medulla oblongata, rendering it incapable of transmitting more feeble reflectorial impulses. In his experiments to determine the functions of the brain in frogs, Goltz made a crucial incision through the skin of the head, detached the calvaria piecemeal, removed the cerebrum, retaining the optic nerves through their whole course, and then sewed the skin neatly together again. Healing of the wound rapidly occurred, and the animals were easily retained alive by artificially feeding them. Such animals, as a rule, made no spontaneous movements. When left fasting, they remained immovably fixed to the same spot, ultimately drying up to a mummy. When placed under a bell-glass with various insects, no efforts were made to seize them, nor was any movement observed, unless a large beetle crawled over them. They acted like a reflex apparatus bereft of all intelligence; yet evidence was obtained that some intelligence still remained, and that, consequently, the brain is not the sole seat of consciousness. Thus, if a needle be run into the leg, the animal springs aside in such a manner as to avoid striking against obstacles, and that this is done intentionally may be shown by changing the

position of the latter, and again pricking the animal, when it will leap in another direction. Again, if a mutilated animal be placed on a piece of board which is slowly tilted, it will climb to the upper angle, and make efforts to preserve its balance. The organ or centre by means of which the fine adaptation of muscular effort required to preserve the balance of the body is effected, lies he believes in the lobi optici, since if these be removed, though the animal will still actively spring forward on irritation, yet its power of maintaining the equipoise of the body is materially impaired: it alights or rests on the dorsum instead of the sole of the foot, and in the above experiment on tilting the board, it makes no effort to preserve its equilibrium. The same phenòmena are observed also if the optic lobes are cut or injured without removal of the cerebrum. Animals from which the cerebrum and optic lobes have been removed, and which, therefore, still retain the medulla oblongata, spinal cord, and cerebellum, right themselves if laid on their backs, and if their limbs are fastened together make the same efforts as unmutilated animals.

8. The researches of M. Prevost have chiefly been made upon the dog, but he has also examined the ganglion in cats and in some rodents. He notices that its anatomical investigation in man is difficult on account of its being concealed by the base of the pterygoid apophysis, and because it is invested by much firm areolar tissue. The following is a translation of the results at which he has arrived: 1. The sphenopalatine ganglion receives on one side afferent and gives off on the other efferent branches. The greater part of the afferent branches only traverse the ganglion without breaking up in its interior; but many of the efferent branches are joined by filaments proceeding from ganglion cells contained in the ganglion. Some of the afferent nerves furnish branches which are lost in the interior of the ganglion, contributing to form a kind of plexus of fibres which decussate in various directions in it, and are connected with nerve-cells. In the dog, the greater part of these branches are supplied by the vidian nerve, together with a branch of the superior maxillary nerve, whilst a few proceed from the naso-palatine nerve. In the dog there is a communicating branch between the superior maxillary nerve and the vidian, but this does not exist in the cat. The nervous filaments which in the cat proceed from the naso-palatine nerve to the ganglion, probably represent, in fact, the several nerves, which in the dog come from this branch. The nerve-cells give origin to fibres some of which unite themselves to efferent trunks, whilst others emerge separately from the ganglion and form nerves of a grey colour. The nerves of ganglionic origin (ganglionic branches) have not been satisfactorily demonstrated in man, though we may conclude from analogy that they are present. These ganglionic nerves may be divided into three series: The first are distributed to the mucous membrane of the nose; the second, to the internal maxillary arteries; the third pass towards the fibro-muscular tissue of the orbit; but it is doubtful whether they are distributed to this tissue, since experiment shows that they do not influence its movements. It is more probable that they are distributed to the adjoining vessels,

but it has been found impossible to follow them beyond the tissue in question. The nervus petrosus superficialis major arises from the facial and contains no filaments originating in the ganglion, since after ablation of the ganglion no degeneration occurs in it. The vidian nerve furnishes filaments to the muscles of the velum palati, to the spheno-palatine ganglion, and to the nerves of the nasal fossæ, but gives none to the anterior palatine nerve, as is shown by the study of the nervous degenerations consecutive to the ablation of the ganglion, or after section of the superior maxillary nerve, or of its naso-palatine branch. The results of numerous experiments on ablation of the ganglion have shown that as M. Claude Bernard stated, it is destitute of sensibility and that its removal causes no change in the nutrition of the nasal mucous membrane or in the sense of smell. The effects of electrical excitation are, first a remarkable elevation in the temperature of the corresponding nostril; secondly, an abundant discharge of transparent serosity by this nostril; and thirdly, absence of all phenomena in the eye. Results essentially similar to these have been obtained from experiments on dogs, under the influence of curara, in whom artificial respiration was maintained. Numerous experiments made in conjunction with M. Jolyet have demonstrated the important physiological *rôle* played by the fibro-muscular tissue of the orbit in the projection of the globe of the eye, when the superior extremity of the great sympathetic trunk is excited by the electrical current. That the phenomenon of projection of the eye is really produced by this tissue, as was first stated by H. Müller, is certain because in experiments made on curarized dogs in whom artificial respiration was maintained, it was found that exophthalmos was produced by the direct irritation of this tissue, and because no projection of the globe occurred after the complete division of the tissue. Lastly, M. Prevost believes he has been able to prove that it is not through the intervention of nervous filaments emanating from the spheno-palatine ganglion, that, as was stated by H. Müller, the great sympathetic acts on the fibro-muscular tissue of the orbit, since this phenomena persists after section of the vidian nerve, and even after the complete ablation of the spheno-palatine ganglion.

9. In this essay M. Rouget draws a distinction between the corpuscles of Krause and those of Meissner, on the one hand, and those of Pacini on the other. The former he contends are formed by one or more nerve-tubes, which as they reach their termination cease to possess a medullary envelope. The cylinder axis still accompanied by a very fine prolongation of the connective tissue of Schwann with its nuclei, expands after a course of variable length into a mass of nervous substance furnished with plasmatic nuclei. Generally speaking, the nerve-tube forms a series of close coils around the central mass, the direction and relations of which have, in consequence of the modes adopted for their microscopical investigation, only been dimly and imperfectly observed by Meissner, Kölliker, and others. M. Rouget points out the analogy that obtains between this mode of termination of sensory nerves, and that observed in the termination of motor nerves.

10. A. Rauber holds that the deeply-seated Pacinian bodies are sensory organs for the muscles. He has counted their number and distribution in man, and finds them as follows: The total number is 2142, of which there are 414 in the hand, 161 for the forearm and lower end of the upper arm, 12 for the shoulder, 275 for the foot, 138 for the leg and lower part of the thigh, 5 for the hip, and 46 for the trunk.

11. M. Lombard, whose essay is also published in English, though we have not had an opportunity of seeing it, is stated by M. Brown-Séquard to have obtained the following results: 1. That slight changes of temperature occur frequently in the skin of the head, with the brain at rest, as indicated by a thermo-electric apparatus; the amount, however, is less than 100th of a degree Cent.; yet this is important, as such changes are not observed in the limbs. All circumstances exciting the attention and producing cerebral action, as a noise, the sight of a person or object, cause an appreciable elevation. Active intellectual exertion produces still more marked effects, though even then the exaltation does not amount to 1-20th of a degree Cent. Elevation of temperature occurs also under the influence of emotion, as in reading an interesting subject with a loud voice. It is certainly neither to changes in the movements of the heart nor to muscular movements that the elevation of temperature is attributable. During energetic intellectual work the temperature of the members falls as much as one-fourth or one-half of a degree Cent., partly, no doubt, owing to the immobility of the body. It is at the occipital region that the changes in temperature are most apparent.

PART III.—SKIN—URINE.

1. M. LASCHKEWITSCH: *On the causes of the Depression of Temperature which results from Suppression of the Dermal Perspiration.* (In Reichert's Archiv, 1868, Heft 1, p. 61.)
2. BERNHARDT RITTER: *On the behaviour of the Skin in Baths.* (In Vogel and Beneke's Archiv des Vereins für Wissenschaftliche Heilkunde, Band iii, 1867, p. 103.)
3. J. WEIGELIN: *On the Excretion of Urea.* (In Reichert's Archiv, 1868, p. 207.)

1. Laschkewitsch observes that the fall of temperature in animals whose skins have been varnished is well known, and believes that this is to be referred to their losing their heat more rapidly, consequent upon the hyperæmic condition of the blood-vessels of the skin and subcutaneous connective tissue. Varnishing the surface acts just in the same way as paralysis of all the vascular nerves. Hence, therefore, the smaller the animal the sooner it dies; whilst, on the contrary, if a varnished animal (rabbit) be wrapped in wool and kept warm no ill result follows. The cause of death, therefore, is not, as stated by Gerlach and others, asphyxia.

2. We have only space here to communicate the principal results

obtained by Ritter in his experiments to determine the eliminative and absorbent powers of the skin of man, in baths of various kinds. He asks, 1. What does the skin give off to the fluid of the bath? and replies as follows: The elimination of gases by the skin continues during immersion, and the water takes up both nitrogen and carbonic acid. The temperature, however, must not be so low as to put a stop to the secretory activity of the skin. The elimination of chloride of sodium and of shrunken and withered epithelial scales which also occurs, is not to be regarded in any light as a vital, but merely as a physical act, these separating because they are loose, that appearing because the water washes out and cleanses the irregularities of the skin from the dried up remains of the sweat. The elimination of albumen from the skin never takes place, and if it be found its origin is to be regarded as purely accidental. (This statement is in opposition to observations of Alefeld and Winkler). He asks secondly, "What does the skin take up from the bath?" and replies, that the absorption of material from the bath fluid through the skin can only occur by osmosis, and he remarks that since every process of osmosis requires for its due performance the thorough moistening of the diosmotic septum by one or both of the fluids, so the activity of the osmotic interchange observed, will be in inverse proportion to the thickness of the septum. But the skin is not only very thick, but the deeper layers are impregnated with the sebaceous secretion, which contains about 30 per cent. of fat, and hence he replies to the question of "What does the skin absorb from the bath?" decisively, "Nothing at all." Some experiments upon the above subject have also recently been made by Reveil, which like those of Ritter led to a negative result. (See 'Wiener Med. Wochenschift,' No. 39, 1866.) Reveil found that after baths containing from 4—12 grammes of arsenic (ʒj—ʒiii), no trace of arsenic was perceptible in the urine. In one instance he remained in a bath containing more than an ounce and a half of arsenious acid for nearly three hours without any trace of the poison appearing in the urine, though it was examined systematically for four days. With a foot-bath of three pints of water and 200 grammes of chloride of potassium, no particle of the salt was discovered in the urine. Nor was any portion of cyanide of potassium found in experiments when one two, and even 500 grammes had been present, even though some carbonate of soda had been added, which, according to O. Henri, promotes the process of absorption. With iodide of potassium in ten experiments with various quantities, it was detected in small quantities in the urine and saliva on three occasions. The conclusions at which Orè arrives are, on the whole, similar to those of Ritter. He states that the augmentation of the weight of the body after bathing when it occurs (for it is exceptional), is too small to admit of its being adduced as an argument in favour of cutaneous absorption. Moreover, the saline substances, iodide of potassium, yellow prussiate of potash, chlorate of potash, arseniate of soda, &c., in solution, not being detectable in the urine or saliva; and the vegetable substances, belladonna, digitalis, &c., exerting no influence on the circulation or innervation, renders it impossible to admit that the skin possesses in the bath the faculty of absorption. Lastly, simple mineral or medicated baths have only an *action of contact* which will vary with the nature of the substance in solution.

3. J. Weigelin believes he has proved that the excretion of urea by muscular exertion undergoes a very considerable increase, which appears especially distinctly in the period of rest following work. The increase amounts to nearly 50 per cent. By voluntary persistent contraction of the muscle the subsequent increase is smaller, but the augmentation of the production of urea during the period of exertion is then much greater than when the muscle works by alternately shortening and lengthening. Heidenhain had already *shown* that the muscles of frogs prevented from shortening, were more acid than if they were allowed to contract, on which account he admits that a greater amount of disintegration of tissue takes place in the former case. Weigelin's experiments substantiate this proposition, viz., that unproductive muscular exertion produces much more urea than that which is associated with work. He also refers to and corroborates Leber's[1] experiment, that active frog muscles are much less fatigued when weights are lifted than when their shortening is prevented.

REPORT ON MIDWIFERY.

By Robert Barnes, M.D. Lond., F.R.C.P.,

Obstetric Physician to St. Thomas's Hospital, &c. &c.

I.—The Non-Pregnant State.

1. *On Electricity as an Anæsthetic after Intra-uterine Cauterizations.* By M. V. Revillout.
2. *Acupuncture as a means of Diagnosis between Fibrous Polypi of the Uterus and Inversion.* By Dr. Guéniot.
3. *A Case of Chronic Inversion of the Uterus.* By Dr. v. Scanzoni, Scanzoni's Beiträge, 1868.

1. M. Revillout says that to allay the pain which sometimes ensues upon intra-uterine cauterizations, he has successfully used electrisation as follows: A pledget of lint strongly soaked in strong brine is placed on the uterine neck; to this a copper stem is applied and placed in connection with an induction apparatus.—*Arch. Gén. de Méd.*, Sept., 1868.

2. Acupuncture as a means of diagnosis of uterine tumours gives indications as to sensibility, resistance, hardness of tissues, and the

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[1] 'Zeits. f. Rat. Med.,' bd. xviii, p. 262.

greater or less vascular development. In a woman aged fifty-six a second tumour was discovered immediately after the removal of an uterine tumour, the place of which it assumed. The closest examination left it uncertain whether this was a second fibrous polypus or a partial inversion. The sound penetrated a short distance all round the tumour. Puncture caused no pain. It was concluded to be a fibrous polypus, which was accordingly removed with a good result.—*Arch. Gén. de Méd.*, 1868.

3. Professor Scanzoni relates with great minuteness an interesting case of chronic inversion of the uterus. The subject had borne three children normally, the placenta being always cast without assistance. Several years after the last labour she began to suffer from metrorrhagia, and mucous discharges, often offensive. A fleshy body came through the vulva. This was replaced. The discharges increased so as to render the patient's position critical, and she was admitted into the Wurzburg hospital. The tumour in the vagina had been twice mistaken for a polypus. Scanzoni insists upon the following points in reference to the diagnosis: 1. The impossibility of passing sounds variously curved so high above the tumour as to pass into the uterine cavity. 2. The evidence of a soft pouch surrounding the stalk of the tumour. 3. The possibility of feeling by means of a finger in the rectum the point of a catheter passed into the bladder, and thus proving the absence of the uterus from its proper place. 4. The circumstance that, either with or without the above-named manœuvre, a fan-like body, small below, spreading out above, is felt above the vaginal roof, and which may on careful examination be recognised as the packet of Fallopian tubes, ligaments, and vessels proceeding to the sides of the pelvis; and lastly, the possibility by dragging down the tumour, of unfolding the folds surrounding the stem, so as to be convinced that the upper part of the stem is directly continuous with the roof of the vagina.

Scanzoni determined to remove the uterus. He applied Maisonneuve's écraseur; and having tightened this round the stalk, he cut off the uterus with a bistoury below the constriction. The chain was left on, perchloride of iron was applied to the stump. The chain fell off on the fourth day. Symptoms of peritonitis set in, and the patient died on the seventh day.

On section, pelvic peritonitis was found. Douglas' pouch was filled with purulent fluid; the os uteri was gaping, opening directly into the peritoneum. The amputated uterus enclosed a cavity, out of which the stumps of the broad ligaments, Fallopian tubes, and several vessels projected. In the anterior wall of the uterus was a hard fibroid tumour the size of a pigeon's egg. Attempt to reinvert the uterus with the fingers failed.

Scanzoni tabulates 58 cases under heads: The first includes 16 cases of removal of the uterus by ligature, ending successfully. The second includes 7 cases of removal by ligatures, ending fatally. The third includes 2 cases of excision ending successfully. The fourth 6 cases of excision ending fatally. The fifth includes 22 cases of removal by ligature and excision ending successfully; and lastly, 5 cases of removal by ligature and excision ending fatally. He rejected the idea of trying to reduce the uterus, successfully practised by Tyler Smith, on the grounds that the attempt must cause great irritation of the vagina, and that hitherto its reduction had never succeeded when the inversion was caused by a tumour in the uterine wall.—*Scanzoni's Beiträge*, 1868.

II.—PREGNANCY.

1. *On Uterine Hydatidiform Disease.* By T. M. MADDEN, M.D.

1. Dr. Madden relates two cases of cystic disease of the ovum, and cites the opinions of various authors as to the nature and origin of the disease. He inclines to the conclusion that it may arise in virgins and chaste women, that is, independently of degeneration of placenta. He thinks that some cases may be accounted for on the supposition that a monstrous growth or morbid action may occur in one of the Graafian vesicles, and that this hydatidiform disease of the unimpregnated ovum may be in some way connected with ovarian disease.—*Dublin Quart. Journ. of Med. Science*, Nov., 1868.

Hydatid Mole in adhesion to the Uterus; Intraparietal and Intravascular Development of the Chorion Villi. By Dr. v. JACOBSKY and Prof. WALDEYER.

Referring to a case of interstitial destroying mole-formation by R. Volkmann ('Virch. Archiv,' Jan., 1868), Drs. Jacobsky and Waldeyer relate the following: A woman had had three children; every time, the placenta had to be removed by art, on account of strong adhesions; a subsequent abortion required no advice. In December, 1866, she was complaining of repeated hæmorrhages. No enlargement of the uterus was detected. At the end of February, 1867, a profuse hæmorrhage occurred. The uterus was now felt half way between umbilicus and symphysis; a soft pulpy mass was felt inside. The patient died. There were simple cysts in both ovaries; no fresh corpus luteum. The uterine cavity was nearly filled with a hydatid mole fast adhering to its walls. It was hard to remove the mass, since many of the bladders were buried deeply in the uterine substance. Lacunæ were left in the muscular tissue, so that the surface resembled the inner surface of the heart; in some places, the lacunæ were so deep that their boundary seemed to be the peritoneum. When a section was made, small bladdery villi on long stalks were seen in the wall; these were ascertained to be connected with the villi proceeding from the inner surface of the uterus. Following these branches with the scissors, they led into large venous vessels which could be traced into the margins of the uterus; and in these undoubted venous branches, free floating villi were seen. Only in one spot of the right half of the uterus was there a portion of tolerably healthy decidua. The mole itself contained a cavity clothed with a smooth membrane from whose outer surface the degenerated chorion-villi everywhere proceeded. No trace of fœtus was found.—*Virchow's Archiv*, 1868.

III.—Labour.

1. *On Labour in Contracted Pelvis* (in Report of the work of the Breslau Lying-in Institution). By Dr Spiegelberg.
2. *On the Clinical Practice to be pursued in Cases of Pelvic Contraction, to the extent of 7½ centimeters.* By Aurelio Finizio.
3. *A Case of Triplets.* By Dr. Scharlau.
4. *Studies on Gastro-hysterotomy.* By Giuseppe Testa.
5. *Permanent Uterine Cavity after Delivery.* By M. C. Carcassonne.
6. *Three Cases of Puerperal Convulsions.* By James Cappie, M.D.
7. *Note in contribution to the History of Ruptures of the Uterus during Labour.* By Jacques Jolly.

1. Taking the rule of Litzmann that a pelvis measuring 3·50″ and under is a contracted pelvis, Dr. Spiegelberg found 83 labours with narrow pelvis in 597. This very large proportion, 13·9 per cent., is explained by the poverty of the lower classes in and near Breslau, and the custom of too early resort to hard work. The greater number of the narrow pelves were simply contracted in the conjugate diameter, the so-called simply flat and ricketty pelvis: but more advanced forms of ricketty pelvis presented themselves, two being uniformly contracted, and there were two exquisite cases of obliquely-contracted pelvis.

The 79 ordinary cases are subdivided in groups: The 1st with a conjugate under 3·0″; 11 labours. In 9 aid was required: 2 were delivered by premature induction of labour; 3 were delivered by turning; in one of these the head had to be perforated; 1 was delivered by forceps, 3 by perforation: 2 of the mothers died; 4 children were born alive. 2nd group: conjugate 3·00″ to 3·25″; 45 labours. In 31 cases labour was spontaneous; 27 children living; 4 were delivered by turning, 7 by forceps, 3 by perforation: 6 mothers died; 3 children were born alive. 3rd group: conjugate 3·30″ to 3·50″; 23 labours. In 20 labour was natural; of these 19 children were alive; 2 were delivered by turning, 1 by forceps. All the mothers were discharged well. The two cases of oblique distortion belonged to contractions of extreme degree. In one premature labour was induced. The child died during birth; the mother died of metro-lymphangitis. The pelvis is preserved. The origin of the deformity was traced to injury of the thigh of the side on which the pelvis was deformed. In third year the patient had suffered a comminuted fracture of the tibia, resulting in a pseudarthrosis and permanent shortening of the right leg. The right iliosacral joint had undergone synostosis; there was atrophy of the os innominatum. The other case, one of coxalgic oblique deformity, was also delivered by premature induction. The child died; the mother recovered.—*Monats. f. Geb.*, Oct., 1868.

2. In an inaugural thesis Dr. Aurelio Finizio discusses, and expounds what may be considered to be the generally accepted doctrine of the Italian school, as to the management of labour in cases where the pelvis is contracted below 7½ centimeters (= nearly 3 inches). He says there are two schools. The first, the older, represented by Prof. Felice de Renzis, opposes provoked abortion on account of pelvic deformity, and admits the following practical indications:—1. From 3″ to 2·50″, the fœtus being alive, symphyseotomy and premature labour; the fœtus being dead, cephalotomia and cephalotripsy. 2. From 2·50″, the child alive, Cæsarian section; the child dead, cephlalotomia or cephalotripsy. 3. Below 1·50″, the child dead or alive, Cæsarian section.

The modern school takes the following indications:—If the woman comes under observation before the 7th month:—1. From 3·00″ to 2·50″, artificial premature labour. 2. Below 1·60″, obstetrical abortion. At term of gestation:—1. From 3·00″ to 2·50″, forceps as an instrument of traction and compression. 2. From 2·50″ to 2·16″, cephalotripsy. 3. Below 2·00″, gastro-hysterotomy, in extreme cases.

Dr. Finizio says the forceps is preferable to turning when the pelvis measures 3·00″. Contrary to the current Italian precept, he applies the blades in the sides of the pelvis. Cephalotripsy can be executed between 2·50″ and 2·00″. In discussing the induction of labour, Finizio lays down these rules:—1. To time the operation in coincidence with the menstrual nisus. 2. Not to cause congestion of the lower segment of the uterus by Kiwisch's irrigation, this having frequently caused death. 3. To dilate the cervix gradually by dilators. He prefers that of Lemenat des Chénais to Barnes' and Tarnier's.—*From Analysis in Annali Univ. di Medecina*, 1868.

3. Dr. Scharlau relates a case of triplets of an unusual kind. A first child, presenting by hand, was extracted alive by forceps. The second, presenting by breech, was also delivered alive. Two completely separate placentas were removed. The uterus contracted; the patient seemed well. Sixty hours afterwards, expulsive pains set in, and a mass was expelled. This was an intact ovum with very thick shaggy chorion, and a small placenta. The cavity contained stinking liquor amnii and a fœtus of the size of three months' development. The remarkable feature was the presence of liquor amnii in the third ovum, since the destruction and mummification of one ovum, during the development of the other, is commonly preceded by the escape of the liquor amnii.—*Monats. f. Geb.*, Oct., 1868.

4. In an elaborate work on the Cæsarian section, Dr. Testa discusses the history, statistics, and other points; and especially investigates the following:—In some cases the wound in the uterus

s found, after death, gaping, in others closed. Testa set himself to discover upon what this difference depended. He minutely examined the muscular structure of the uterus. He found that when the incision is so made as to fall in the line of direction of the fibres, the lips of the wound will be maintained in apposition; but that when the wound cuts across all the layers of muscles, gaping must follow. The gaping will be the greater, the longer the fibres are which are divided; and will increase with the uterine contractions; and since the external muscular fasciculi are longer than the internal, it is clear that the retraction of the first will predominate, and cause the divergence of the margins of the wound. Thus he observes that when the incision fell upon the fundus, cutting the transverse and oblique fibres proceeding from the Fallopian tubes, the wound gaped, owing to the retraction of these fibres towards their points of origin. And when the incision was made transversely from one of the sides, so as to spare the longitudinal fibres of the tubes, the edges of the wound could be drawn together. In the lower third of the uterus, wounds made in the direction of the long axis, would present the margins in contact, the longitudinal fibres being spared; but since the neck becomes distended and relaxed during the latter months of pregnancy, it follows that the borders of the wound are kept apart by the divided and stronger transverse fibres. The gaping would be greater if the incision was oblique, so as to divide all the orders of fibres. It is true that the anatomical structure of the middle third is the same as that of the lower third, but at this point the longitudinal fibres not having suffered distension, and preserving their tonicity, are fit to maintain in contact the margins of wounds made according to their direction.

The application of these researches follows:—1. Incisions in the uterus made in the axis of the body, in the upper third, remaining open, give rise to escape of lochia into the abdominal cavity, setting up peritonitis. 2. This may be avoided by making the incision transverse and lateral, but this is to be avoided on account of the risk of dividing large vessels. 3. Oblique and longitudinal incisions in the lower third equally dispose to effusions and peritonitis. 4. Testa recommends to make the incision transversely, and on one side, a little above or below the insertion of the tubes, so as to avoid an order of fibres (the longitudinal of a tube before they become oblique), and thus avoiding all obstacle to the approach and union of the lips of the wound. He relates that Dr. Cocchi, of Rome, having followed this advice, saved mother and child. Dr. Testa then examines the various modes of applying sutures to the incised uterus. He objects to most of those proposed; and suggests one of his own. It consists in inserting two long needles at the level of the upper angle of the incision, at a distance of four lines from it, from without inwards through the abdominal parietes; then in making each needle penetrate the thickness of the corresponding lip of the uterine wound throughout its whole length, and always at the same distance from the margin; this done, the needles are brought out from within outwards, at the level of the lower angle of the wound. Care must be taken not to pierce into the cavity of the womb. Then a thread is twisted round the upper ends of the needles, and another round the lower ends; the rigidity of the needles suffices to keep the edges both of the abdominal and uterine wounds in apposition. Then straps of adhesive plaster are applied. Thus parallelism is preserved between the wounds; the escape of matter into the peritoneum is avoided; and the protrusion of intestines is prevented.—*Analysis in the Annali. Univ. di Medicina,* 1868.

5. M. Carcassonne has applied an instrument of his construction designed to measure the force of the uterine contractions, to the determination of the existence of a cavity in the uterus after delivery. It consists of a canula, having a small bladder attached to the end, which is introduced into the uterus. The end introduced can be made to describe arcs of circles in the uterus without meeting resistance; and again, if water be injected into the bladder, and this is not then equal in size to the capacity of the cavity, then by moving against the uterine walls, one gets the conviction that these are not in juxtaposition.—*Gaz. Med.*, Oct., 1867.

6. Dr. Cappie relates three cases of convulsions occurring near the natural term of gestation. He practised early delivery, dilating the cervix uteri with the fingers, and applying the forceps before the dilatation was complete. He urges that this mode of delivering the patient as quickly as possible is the proper treatment to adopt. He found little or no service from bleeding or chloroform, and says the fits were not excited or increased in severity by the operator's proceedings.—*Edinb. Med. Journ.*, Oct., 1868.

7. M. Jolly saw last year in the Maternité two cases of rupture of the uterus, in both of which the contractions went on even after the accident. In one case they were sufficiently energetic to expel the child. This circumstance seemed to be so rare, that he searched for other cases, and collected the histories of twenty-three cases in which the returns showed contractile energy after rupture. He discusses the explanations given to account for the continuance of the contractions, and successively rejects them as contradicted by clinical facts. Thus Baudelocque's theory that the fœtus present in the uterus excites it to contract is disproved by cases cited where contraction occurred, even after the child and placenta were expelled, and others where the contractions ceased, although the child remained in utero. Another theory urged by Ramsbotham is that a large rent caused suspension, a small one contraction. Jolly adduces cases in contradiction of this also.

Nor does explanation lie in the seat of the rent, as Pajot suggested, for contraction has been absent and present in ruptures at the fundus and at the cervix.

Jolly insists upon a diagnostic sign, noticed by McClintock. This is the formation of a tumour in the iliac region, formed by a mixture of blood and gas, recognised by a sound of crepitation and a sensation of emphysema. Another sign is the formation of a tumour of effused blood in the hypogastric region, simulating the distended bladder, and not disappearing on catheterisation. These are not constant, but valuable when present. The conclusion is that suspension of contractile energy being no certain or constant sign of rupture, we must study carefully all the signs, not so much in isolation as in their accumulation.—*Archives Gén. de Méd.*, 1868.

IV.—THE PUERPERAL STATE.

1. *Miliary Puerperal Fever.* By Professor GRILLENZONI.

1. The Professor and Director of the Maternity at Ferrara, in an interesting report of that institution, relates some cases of miliary fever which appeared to present an epidemic character. They occurred in the winter and spring of 1865-6. In the course of this epidemic, there happened also a distinct case of ordinary puerperal fever. As the miliary fever is not now common in this country, it may be interesting to cite the description of the disease as it occurred in the Ferrara Lying-in Hospital. A healthy woman had a healthy labour on the 31st of January. She was seized with fever on the fifth day, beginning with a rigor similar to ague, and ending with copious sweatings. On the sixth day the miliary eruption appeared; on the ninth there was dysuria. The fever re-appeared on the eleventh day, and fresh eruptions attended it. The fever was renewed from day to day with copious sweating and outbreak of miliary vesicles. After the fourteenth day the fever did not return, except on the seventeenth. A phlegmonous swelling in the meantime appeared on the right buttock, and turned to abscess, which was opened on the twenty-fourth day of illness; pus and dead adipose cellular tissue were let out. She was quite well at the end of four weeks. The other cases ran a similar course, the chief features being repeated attacks of fever ushered in with shivering, profuse sweating, and outbreak of miliary rash following, and a tendency to the formation of abscesses. The eruption consisted in small vesicles with a red base appearing on the chest, arms, abdomen, and thighs. The patients were treated with quinine and Dover's powder, and it seemed, with good effect.—*Annali Universali di Med.* Milano, 1868.

The following memoirs, for want of space, are referred to by title only:—

On Fatty Degeneration of the Placenta. There are some interesting observations on this subject in a memoir "On Fatty Degenerations," in Bartholomew's Hospital Reports, 1868. By E. L. ORMEROD, M.D.

Chorea in Pregnancy. An interesting case of recurrent chorea in two successive pregnancies is reported in the *Lancet,* Oct., 1868. By Drs. H. THOMPSON and HALL DAVIS.

Case of Choreic Convulsions and Tænia occurring in Pregnancy. By W. C. BLACKETT. *Trans. of Newcastle Obstetr. Soc.*, 1868.

On Twisting and Knot Formation of the Navel Strings in Twins. By Dr. P. Müller.—*Scanzoni's Beiträge zur Geburtskunde*, 1868.

A Contribution to the Study of Lateral Hermaphroditism. By O. v. Franque.—*Ibid.*

On Utero-Vaginal Atresia and Stenosis. By Dr. P. Müller. A case is related and references are made to others recorded.—*Ibid.*

A Malformation caused by Amniotic Bands. By Dr. Jensen.—*Virchow's Archiv*, 1868.

On the Structure of the Cephalrotibe. By Dr. Matthews Duncan.—*Edin. Med. Journ., Dec.*, 1868.

CHRONICLE OF MICROLOGY.

By J. F. Streatfeild, F.R.C.S.,

Surgeon to the Royal London Ophthalmic Hospital, and Ophthalmic Surgeon to University College Hospital, &c.

Nerve-corpuscles of the Conjunctiva and of Touch.—M. Ch. Rouget says (Académie des Sciences), in examining the zone of ocular conjunctiva next the human cornea, having first removed the epithelium by maceration in an acidulated fluid, there are found, dispersed in groups, little rounded bodies, appended like grains to the ends of double-contoured nerve-tubes. The nerve-tube which is their pedicle makes a kind of annular buckle, whose extremity, curving in towards the centre, expands, and is continuous with a spheroidal mass of granular substance. This is very refractive, and contains some oval nuclei. If one supposed the buckle unfolded from the nerve-tube, the granular mass which essentially forms the corpuscle would appear as a globular enlargement of the cylinder axis. The supposed capsule of nucleated connective-tissue figured by Krause and Ludden is only an appearance due to the fact of the nerve-tube wrapping around the central corpuscle. This wrapping is generally confined in the smallest corpuscles to a single turn; sometimes even this is incomplete, whilst around the larger corpuscles this nerve-tube describes two, three, and sometimes even as many as ten turns.

An individual corpuscle may receive two or three nerve-tubes. It is not always possible to observe the point at which the end of the nerve-tube is conjoined with the granular substance.

If, since the discovery of Meissner and Wagner, the existence of corpuscles of touch has been certified by all observers, it is not at all the same thing as to the structure of these corpuscles, which in fifteen years have stirred up controversy, which still exists, concerning them.

The researches of M. Rouget have led him, not only to refute completely the data of Kölliker, but, on the contrary, to accept most of the facts already acknowledged by Meissner and Leydig. In this corpuscle of touch altogether, in the different layers of which it is composed, there is nothing else to be remarked than a particular arrangement of the ultimate extremity of the nerve-tube. It exactly reproduces, in a greater degree of complication, the fundamental type which the nerve-corpuscles of the conjunctiva show in an extremely simple state; that is to say, a nerve-tube wrapping up, winding regularly round a central mass of nerve-substance identical with that of the ganglion corpuscles, of the end-plates, true expansion of the cylinder axis, deprived of the protective and isolating layers which doubly envelope it in the nerve-tube.

It is by an error in observation that one has thought to have seen and to have delineated terminations of nerve-tubes by loops on the surface of the corpuscles.

Departing from the base of the papillæ, the nerve-tubes given off from the subcutaneous net-work are directed towards the axis, and reach the touch-corpuscle, sometimes at its lower extremity, at others at its middle part, or again skirting the borders or traversing the surface, they reach the neighbourhood of the upper extremity.

In attentively observing the point at which the double contoured tube seems to end, one observes that, losing at this point the medullary layer and the so characteristic refraction which belongs to it, the nerve-fibre grey and pale, generally reduced in size, but yet still endowed with some refraction, traverses between the transverse streaks of the corpuscle, and sooner or later, penetrating the thickness of the cortical layers, disappears; at other times the nerve-tube, losing its medullary layer, divides into three or four slenderer filaments, which pursue a transverse direction, and manifestly enter into the system of the transverse fibres of the cortical layer.

In some corpuscles, whose different parts have been artificially or are naturally dissociated and separated from each other, one can follow out the manner of the wrapping up of the nucleated grey fibres, and certify their direct continuity with the double-contoured tubes. In the central mass of the corpuscle the nucleated grey fibres are wanting, as are the medullary layer tubes. Wagner and Meissner have figured, as sections of nerve-fibres, in transverse section of the central mass of the corpuscle, nuclei dependent on this substance, which Kölliker, for his part, wrongly describes as a connective blastema, homogeneous and transparent, without nucleus formations. Really this central mass is made up of a finely granular substance, very refractive, nucleated, and identical with that which forms the central mass of the nerve-buttons of the conjunctiva.—*Archives Générales de Médecine*, June, 1868, pp. 743-5.

Enamel and Dentine.—Dr. Beale differs from the views of Huxley concerning these structures—that they are dermal tissues, and situated beneath the basement membrane. He concludes, from the position of the vessels, as regards the dentine, the manner of growth of both tissues, and the fact of their origin in a collection of unquestionably epithelial cells, that both enamel and dentine are more nearly allied to epithelium than to any other tissues of the body, and that both are developed on the surface of the basement membranes. The masses of germinal matter concerned in the formation of the enamel and dentine are imbedded in epithelium, and are arranged in two curved lines, one within the other. The slight interval between these lines corresponds to the line of junction of the enamel and dentine in the fully formed tooth. From this neutral line the two rows of masses of germinal matter move in opposite directions. In the outer one each mass diverges outwards from the neutral line, while the different masses of germinal matter of the inner row converge slightly as they move inwards.

Enamel.—Its formation is well seen in the canine tooth of a young pig. The preparation is injected, the capillaries well filled with transparent blue injection, and the enamel cells, the germinal matter of every one having been well stained with carmine, are very distinctly seen. Each appears as a columnar or cylindrical body, not unlike a cell of columnar epithelium, with an oval mass of germinal matter nearest its distal extremity. As the germinal matter moves outwards from the neutral line above referred to, it forms the column of soft material which is to become the enamel rod. After some extent of tissue has thus resulted, calcareous matter is deposited in that part of the column which was first formed. Several columns can be discerned in which the change has already commenced. The highly refracting earthy particles contrast remarkably with the smooth, faintly granular, organic matrix. The deposition of these earthy salts may be due merely to chemical change, consequent upon the formation of free alkali in this the oldest part of the organic matter. While this process is going on, the germinal matter in each little column is still moving outwards, and forming more organic matter, which, in its turn, becomes calcified. This process continues until the formation of the enamel is complete, when the vessels of the membrane waste. A little uncalcified organic matter usually remains upon the outer surface of the enamel. The markings seen in a transverse section of enamel receive a simple explanation upon this view of the development of the enamel, and the outer uncalcified portion of the rods, when acted upon by acetic acid, swells up; and thus some have been led to infer the existence of a delicate membrane in this situation.

Dentine.—The dentine begins to form before the enamel; but very soon after the formation of enamel has commenced the two operations go on together until a short time before the tooth emerges from the gum, when the production of enamel ceases, while that of dentine is not completed sometimes until advanced age. In certain cases—as, for example, in the canines of some of the lower animals and in the incisors of the rodents—the formation of both structures continues, so that in the teeth of the adult the development of the enamel and dentine may be studied as well as at a very early period of embryonic life in other cases. The oval masses of germinal matter taking part in the formation of the dentine are larger than those of the enamel, and the formed material produced by them appears as a continuous matrix instead of distinct and separate columns. Moreover, instead of each mass forming a separate oval body, a thin line of tissue is drawn out as the mass of germinal matter moves inwards. These lines of soft tissue correspond to what are generally termed the dentinal tubes, and may be forcibly withdrawn. Calcification takes place by the deposition in the matrix of roundly globular masses of calcareous matter, which increase in size and coalesce. A narrow portion of the matrix, extending outwards from each mass of germinal matter, still remains permeable by fluids, and the process of calcification proceeds so much more slowly around this than in the rest of the matrix, that the dentine produced refracts differently, and is harder in texture. The difference in appearance seen in thin sections has led observers to regard this slowly formed layer of dentine as the wall of the supposed "tube." But in the recent state this "tube" is occupied by uncalcified matrix, which can be torn away from the calcified dentine. The calcification of the formed material corresponding to the "tube" gradually proceeds, so that the space or "tube" occupied by soft matter becomes narrower as the dentine advances in age, and at last in many cases the outermost portion becomes completely calcified, in which case there is no "tube" at all, and the tooth is usually thoroughly dried before the section is made. The "dentinal tube" of the prepared specimen results from the desiccation of the organic matter.—*Medical Times and Gazette*, Dec. 5th, 1868, p. 636.

Brain lesion in an insane person.—In a paper containing the results of minute examination of the brains of many cases of mental disease, Drs. Tuke, Rutherford and Skae describe one case of a nature hitherto undescribed. The patient had been a clergyman, aged 65. He had had an apoplectic seizure some years before his admission to an asylum for insanity. He had partially recovered from this, but was paraplegic, indistinct of speech, unsteady in the lower limbs, incoherent in his ideas, but he had no distinct delusion when he was admitted. After a second attack of apoplexy he died. In the post-mortem examination, seventy-three hours after death, various clots and remains of clots were found. The left half of the cerebellum was much atrophied. In the atrophied white matter there were easily seen, with the naked eye, a large number of *semi-opaque* whitish spots, in a thin section which had been hardened in chromic acid rendered transparent by turpentine, and coloured with carmine in the usual way. The spots were almost entirely confined to the white matter, irregularly scattered, generally oval, and some of the large ones, by fusion, lobulated, of all sizes from 1-40th of an inch downwards. The long axis of the oval spots was generally found to lie parallel with the nerve-tubes surrounding them. With a low power and direct light the spots had a somewhat luminous, pearly lustre. When magnified 250 and 800 diameters linear, they were seen to consist of molecular material, with a stroma of exceedingly delicate colourless fibres. Most of the spots had tolerably well-defined margins, consisting of nerve-tubes and blood-vessels curving round them, and looking as if they had been pushed aside by the molecular material. When the section was steeped in ammoniacal solution of carmine, the somewhat fibrous-looking walls of the spots became more deeply tinged than the nerve-tissue lying external. The molecular material of the spots did not, however, attract the colouring matter of the carmine. Magnified 80 or 100 diameters linear, the majority of the spots appeared as if surrounded by red capsules. The molecular matter of which the spots consisted had a yellowish-green tinge, probably derived from the solution of chromic acid with which the tissue had been hardened. A few of the smaller spots were not, however, surrounded by distinct fibrous-looking walls, but the surrounding nerve-tissue appeared gradually to merge into the molecular material. Although many nerve-tubes curved partially round the spots, a solution in the continuity of others could be observed at the margins. The smallest spots visible were about 1-1500th of an inch in diameter: they were seen to consist of a somewhat diffuse molecular cloudiness or milkiness of the nerve-tissue. In dried sections, these spots formed little eminences above the shrivelled nerve-tissue, and could easily be picked out from it with the point of a knife. They were not gritty; did not effervesce with hydrochloric acid, acetic acid, boiling alcohol, and ether produced no alteration of the molecular material. But with strong, pure nitric acid, the whole section, molecular matter, as well as nerve-tissue became more transparent, although spirit of turpentine, while it rendered transparent the nerve-tissue, did not affect the molecular matter. As the molecular material became transparent under the action of the nitric acid, a number of colourless rounded bodies, about the size of a blood-corpuscle, appeared. These rounded bodies could be seen forming by the coalescence of minute transparent drops, apparently the molecular matter rendered fluid by the acid. These drops, coalesced within the spots, could be squeezed out: they were sometimes elongated, even then, showing that the matter of which they were composed was viscous. By repeated pressure on the covering glass and prolonged action of the acid, the viscous matter could be entirely removed from the spots, and nothing but the delicate fibrous stroma left behind. It was a network of very delicate connective tissue—the neuroglia, in fact, from which, however, the nuclei and nerve-tubes had disappeared. The disappearance of the nuclei and nerve-tubes could not have been due to the acid, for this did not cause them to disappear from the healthy tissue: the molecular material must therefore have replaced these two elements, leaving the connective tissue fibres intact. Around the spots so emptied of their molecular contents the abrupt terminations of nerve-tubes could be distinctly seen, showing that a destruction of nerve element had taken place. But nitric acid caused the appearance, from the nerve element, of viscous matter closely resembling, if not really identical with, that formed from the molecular material. Other chemical reagents, caustic potash, boiling ether, boiling alcohol, and an alkaline solution of iodine, with sulphuric acid added, had no other than negative effects. The authors arrive at no certain conclusion as to the mode of development of these spots. The first stage seems to be the appearance of somewhat opaque, white molecular material, which gives rise to a *cloudiness* of the tissue.

There is abundant evidence of what the material is *not*. In theory, this lesion is the result of a kind of growth which leads to the formation of molecular material, which, by encroachment, causes the disappearance of nerve-tissue. Quite unlike that which Rindfleisch has described as the essential change in grey degeneration. There was but one spot in the grey matter in the above instance, and the section was at that part so thick that nothing could be made of it.—*Edinburgh Medical Journal*, Sept., 1868, p. 204.

Gliomatous Tumour of the Brain.—Dr. Lauchlan Aitken brought this case, which he characterises as rare and very perfect, before the 'Edinburgh Medico-Chirurgical Society.' The patient was a man of 50 years of age, a seaman; he became melancholy and forgetful, lost power in his left leg, had then excruciating pain in the region of the right supra-orbital nerve; no paralysis, or convulsions, or loss of sensibility; he answered simple questions slowly and intelligently. He had occasional diarrhœa or hæmaturia. His vision failed little by little until he could only see light from darkness. He died three or four months after the pain in his head began.

Much fluid was found in the left ventricle, a tumour in the right anterior lobe, irregular, soft, somewhat friable and slightly yellow. Around it was grey softening, with disintegrated nerve-tissue, and compound granular cells, and here and there small blood extravasations. Under the microscope the tumour seemed to be composed of a mass of cells of various shapes, some oval, others square, but most of them rounded, and strongly resembling the white cells of the blood, being, however, a little smaller. They were granular in appearance, *i.e.* seemed to contain a number of small nuclei within them. A few minute capillaries, too, ran amongst the cells; but it was only after the addition of acetic acid that the author could see anything like an intercellular substance. This seemed to consist of very fine fibrils crossing amongst the cells in all directions. Towards the edge of the tumour, however, in a few places, he distinctly discovered some nerve-tubes. After the preparation had lain some time in alcohol, he could make out no new constituents, but the intercellular substance had become more distinct. There had been no ophthalmoscopic investigation.—*Edinburgh Medical Journal*, Sept., 1868, p. 225.

Fibroma of the Breast.—In this tumour, says M. Ranvier, are found lacunar cysts, and in these cysts are seen projecting elevations which appear microscopically at first sight to be very like glandular culs-de-sac. But the epithelium of these fictitious culs-de-sac exists on their outer surface, so that they cannot be considered as, in any sense, glandular elements. Virchow also has described them as *fibroma of the breast;* the new productions of connective or mucous tissue which make up the tumour grow as buds within even the glandular canals, and these buds consequently are covered by epithelium. The lacunar cysts are no other than these canals dilated.

The preparations I exhibit, said M. Ranvier, are from one of these cases which are in France improperly called *adenoma*. One of these consists of the membrane of one of these lacunar cysts treated with nitrate of silver, carmine, and preserved in oxalic acid. In this way the pavement epithelium is very well seen. In other preparations I have studied, by impregnation with nitrate of silver, the mucous connective tissue which separates the lacunar cysts. In this way are seen—1st. The blood-vessels with the impregnated epithelium. 2nd. Lymphatics which end pointedly and are directly continuous with the lymphatic spaces of the connective tissue. The plasma cells are very large, stellate, and communicating with each other.

During many months, continued the author, I have studied a great number of tumours of the breast; in all, there was an hypertrophy of the glandular culs-de-sac, and an hyperplasy of their elements, whether the case were one of carcimona or of any other tumour; it is not there that the characters of each tumour are to be sought, but in the modifications of the connective tissue. In tumours of the nature of the above case there is a budding of the connective tissue in the interior of the canal, and this has been considered as glandular.

The lacunar cavities are the primitive canals. Their surface is lined by an epithelium, sometimes pavement, sometimes cylindrical, sometimes simple, sometimes stratified. In even one tumour one may meet with these different varieties of pavement and cylinder epithelium. The tissue itself of the tumour is sometimes fibrous, sometimes mucous. The epithelium often undergoes insensible transformations from the cylinder to the pavement epithelium.—*Robin's Journal de l'Anatomie et de la Physiologie*, Sept. and Oct., 1868, pp. 564-6.

Biliary and lymphatic vessels of the human liver.—Their pathological conditions appear in an enlargement of the biliary capillaries by accumulation of bile as well as in a dilatation of the lymphatic capillaries of the liver by arrest of the circulation in the venous system, allowing a more accurate insight of the disposition of the two kinds of vessels to be obtained. Dr. Alfred V. Biesiadecki says, by *a slight degree of accumulation* the microscope shows the green-colouring of the interlobular tissue caused by the accumulated green-coloured bile, which in a different degree extends the interlobular biliary vessels. There are found at the same time amongst the liver-cells framework numerous longitudinal and transverse sections of mostly very fine canals which subdivide and are enlarged as knots at the places at which they divide, which possess no proper membrane, but only contain the comprised liver-cells and yellow-greenish inspissated bile. In longitudinal section these canals are always bounded by a series of liver-cells, which possess a fine punctate protoplasma, are free of biliary colouring matter, and whose nucleus in the order of the wall of the blood-vessel lies nearer than to the canal. In cross section the canal is ordinarily surrounded by five, but rarely by four liver-cells, and is not round, but presenting as many facets as there were liver-cells surrounding it. Here also is situated the most enlarged nucleus of the liver-cells nearer to the blood capillary wall. The canals described are to contain the enlarged biliary capillaries, because of their much less diameter in relation to the blood capillaries, because of their course within the liver-cells framework, as well as because of the varicose dilatations at the points of division, because of their yellow-greenish biliary-like contents, and specially because of their direct limitation of the liver-cells, without interposition of a special membrane, as is the case with the blood capillaries.

By a higher degree and longer continued accumulation of the bile and increased green colouring of the liver-parenchyma the biliary capillaries are not filled up completely with fluid bile, but with more or less thick, round, rod-like, often branched and by pressure broken-up concretions, which show but little of the contour of the liver-cells next to them. At the same time the liver-cells are filled with abundant brownish-red granular pigment, and even here and there comprise small masses of green colouring matter. The cell nuclei become always larger, finally indiscernible, whereupon the cells diminish and coalesce. But moreover, there follows this shrivelling of the cells, and indeed specially in the places where also within the blood capillaries similar cell-concretions are distinguished as at one time round rods, at other times as large, greenish-coloured, compact, uniform lumps, which do not fill up the whole lumen of the vessel, and are surrounded by blood-corpuscles. These completely homogeneous formations are soluble in chloroform, and consequently cannot be blood coagula soaked in bile. In these conditions the bile may so much the more easily pass over into the blood-vessels, as in general these causes which bring about the compression of the ductus choledochus or hepatic duct exercise also the same effect on the portal vein, and so diminish the force of the blood with the capillaries.

With the formation of biliary concretions atrophy of the liver-cells appears commencing as an enlargement of the nuclei of the cells; after that biliary pigment becomes taken up in the protoplasma; and at last the nucleus becomes indistinct, and the limits of the individual liver-cells obliterated; so that instead of the framework an indistinct mass is left, which includes brownish-red pigment, perhaps decreasing even to one third of the original bulk of the liver framework, but still it is found in extreme atrophy. The atrophy seizes individual lobules here and there, whilst others, situated between them, continue to keep tolerably well. By this the envelope of the liver gives way here and there, and it has the appearance of a cirrhosed liver. By these proceedings the interlobular connective tissue augments, the larger biliary vessels dispersed, in which are, by inspissated bile, dilated even to double their size, and the intralobular connective tissue is interspersed with numerous brownish-red pigment-granules.

But besides this dilatation of the blood-vessels there follows also a dilatation of the lymphatics. The lymphatics dispersed in the capsule of the liver are dilated less than those which pass from the deeper parts of the liver towards the hilus, and are imbedded in Glisson's capsule. They there exhibit an abundant connecting network with ramifications of very considerable thickness, which beyond their valves show knotty extensions. These ramifications discharge in the retroperitoneal lymphatic glands. But also within the lobules the dilated lymphatics are found. Thus one finds the interlacing blood capillaries and liver-cells framework separated by gaps, which resemble the perivascular lymphatic spaces pointed out by MacGillivry. These spaces or gaps surround the blood-capillaries as a sheath, attain a third of the diameter of the latter, and are on the one hand bounded by the thickened capillary wall provided with enlarged nuclei, and on the other hand directly by the liver-cells; only here and there are the two walls united by a transverse fine connective tissue fibre. These spaces are largest where the liver-cells are but little diminished. The atrophy of the liver-cells always becomes considerable so much the more as is taken from the size of the spaces, until at last where the liver-cells framework have become thin brownish red, and with nuclei rarely to be seen, degenerated, almost completely to have disappeared. By the atrophy of the liver-cells and its consequent degeneration of the capillary wall these spaces are not also originated. Also they do not appear by other conditions originated atrophies, as by the pressure of arrested biliary circulation, in the atrophy of old age.—*Schmidt's Jahrbücher*, vol. 140, page 173.

Visceral steatosis by inanition in the new-born infant.—M. Parrot contributes a paper on this subject to the French Academy of Sciences, his object being to point out lesions found in the viscera of infants who in no sudden or violent way die soon after their birth, but by a continuous impairment of all functions.

1. In the arachnoid the lesions consist of small patches, irregularly rounded, ordinarily opalescent, sometimes yellowish, found at the level of the convolutions. They are due to fatty changes of the cells of the connective layer of this membrane.

2. In the brain and spinal marrow the seat of the disease is analogous; it is in the neuroglia, whose cells, infitrated more or less with fatty granules, become sometimes true granular bodies. When, at certain points, these accumulate, the lesion, which is generally only microscopic, becomes visible to the naked eye, as little white hard plates, of chalky appearance. Of all the intracranial organs the corpus callosum is the most considerably affected, especially at the level of its lateral borders.

3. The emphysematous lung, in its alveoli, presents masses of granular bodies, or of oily drops, due to fatty change of the epithelial cells. The lesions, generally microscopic, sometimes appear in the periphery as little opaque patches of a yellowish white.

4. In the kidneys the change affects particularly the convoluted tubules of the cortical substance.

5. The hepatic and cardiac lesions show nothing particular; the latter is generally but little advanced.

For this steatosis of the viscera one can discover no other cause than a deficient, or at least altogether insufficient alimentation, and experiments have confirmed in the strongest way this etiological datum, by the fact, with which we are acquainted, that young animals dying of inanition show changes like those just described.—*Archives Générales de Médecine*, Sept., 1868, pp. 366-7.

Striated muscle—repair of.—M. Maslowski, of St. Petersburg, as a result of his extensive researches as to the reproduction and cicatrization of muscular tissue after wounds, says that the new muscular fibres are developed at the expense of the recent cells which have their origin between the old muscular fibres, which have no part in the new formation of striated fibres. The white corpuscles of the new cells arise in great part from the extravasated blood; but experience does not clearly show whether or no some of them proceed directly from the cells of the connective tissues. The white globules extravasated are in part converted into pus, and for the rest they take part in the formation of cicatricial connective tissue, the results of wounds of the muscles and tendons.—*Journal de Médecine*, Sept., 1868, page 244.

The Atmospheric Germ Theory.—Professor Bennett, of Edinburgh, has published a lecture in refutation of much that has been advanced in this theory. *Omne vivum ex ovo* is untenable. "The idea that there are such things as histological organic forms, which are necessarily indicative of life, must now be regarded as fallacious. In short, we have first molecular depositions in organic fluids; these aggregate to form nuclei, cells, and other tissues; and these again, by constantly breaking down and reforming, gradually elaborate and build up the higher organisms. It would appear that nature is constantly operating in the organic and inorganic worlds by the process of molecular coalescence; that the notion of every particle being necessarily derived from its like is erroneous; and that the law of descent from parents, which we recognise in the higher animals, changes, as we descend in the scale, first to *parthenogenesis*, whereby this direct descent is broken, and ultimately to *heterogenesis* in which it is lost. . . . New life springs from the molecular death of pre-existing tissues and organisms."

The author goes at length into the "Mode of Development of Infusoria." The pellicle or film that forms on the surface of an infusion of any vegetable or animal substance is seen to be composed of a heap of minute molecules, varying in size from the minutest visible point to that of one thirty thousandth of an inch in diameter. Two, and then more, melt together to form a short staff or filament—*bacterium*. There is seen a vibratile movement in the shorter bacteria, and a serpentine movement in the longer ones, whereby they are propelled forwards in the fluid—*vibrio*. Then they all die, and rapidly disintegrate, and thus a second molecular mass or pellicle is formed. In this, rounded masses may be seen to form, which strongly refract light, not unlike pus-corpuscles, or the colourless corpuscles of the blood. These soon begin to move with a jerking motion dependent upon a vibratile cilium attached to one of its extremities, *Monas lens*. In a day or two other cilia are produced, the corpuscle enlarges, is nucleated, and swims through the fluid evenly. Varied forms may now occur in the molecular mass, dependent on the temperature, season of the year, exposure to sunlight, and nature of the infusion, all having independent movements. They have been denominated *Amœbæ*, *Paramecia*, *Vorticellæ*, *Kolpoda*, *Keronæ*, *Glaucoma*, *Trachelius*, &c. &c. One frequently passes into another. Their production is largely dependent on temperature, state of the atmosphere, light, especially the sun's rays, and other physical conditions. At other times the molecular mass gives origin to minute fungi. It is not yet known why sometimes animalcules, and at others fungi, are produced. The higher infusoria are formed secondarily out of the disintegrated mass of the simpler ones. Is the molecular pellicle derived from the air or the fluid—are they precipitated from above, or do they float to the surface from below? In no case has the dust of the air shown any germs. As a tube is formed by a coalescence of cells, so is the minute vibratile vibrio formed by the coalescence of molecules. The vibrios are derived from the molecules, and not the molecules from the vibrios. The more simple infusoria do not originate from cells or minute germs at all, whether in the atmosphere or in the fluid. Living germs are not necessarily the cause of putrefaction and fermentation; neither is it necessary to believe that ferments are living at all—they may be dead. It has been attempted to be proved that in the condensed vapours of hospitals and other putrid localities, vibrios may be found; but that vibrios are the cause of these various diseases is not only not proved, but is highly improbable.—*Edinburgh Medical Journal*, March, 1868, pp. 810-34.

Infusoria in the Blood.—M. Ferrand writes that in infectious diseases it is in the blood itself in which the infusoria are met with; the form and consistence of the red globules are modified, and the white globules are increased in number. One may recognise, by examination of the blood, of what kind of infectious complaint it is a case; thus, the putrid infection and the typhoïd infection are characterised by two kinds of Bacteria, *Bacterium punctum* and *Bacterium catenula*, common to the two diseases, but of different dimensions; the putrid Bacteria, with a width of 16·10000 mm., measuring from 2·1000 to 4·1000 mm. in length; the typhoïd Bacteria having from 4 to 8·10000 mm. in width, with a length of from 1 to 4·1000. In the variolous infection there are varieties which much resemble the *B. termo* and *B. bacillus*.—*Gazette Médicale de Lyon*, November 10, 1867, p. 483.

REPORT ON SURGERY.

By John Chatto, M.R.C.S.E.

On Traumatic Fractures of the Larynx.—M. Fredet, in communicating a case which has come under his notice, expresses his surprise that so little is to be found on the subject of fractures of the larynx in manuals of surgery. A man, æt. 30, having engaged in a quarrel, was seized by the throat by a strong adversary, who, after throwing him on to the ground, kept him there some instants, with his hand on the anterior part of his neck. It was then found that the man could not rise, and was unable to speak, while his face was bloodshot. He was not seen by a medical man until next day, when he was found with a cyanotic face, and labouring under dyspnœa. His neck was emphysematous, but no fracture could be perceived. Under the influence of leeches, the swelling had nearly disappeared, and the breathing had become easier, but on the third day after the accident while endeavouring to get into bed, he suddenly fell dead. At the autopsy a triple fracture of the cricoid cartilage was discovered. The most considerable occupied the median portion of the cartilage, and was as clean as if cut by a knife. The others were situated on the right and left lateral portions. The left arytenoid cartilage was incompletely luxated, and there was considerable œdema of the glottis, cordæ vocales, and epiglottis. The sudden death seemed to have arisen from the displacement of one of the fragments of the cricoid and the corresponding arytenoid, which, riding on the other, completely obstructed the passage of air. Having noted the principal facts of the few cases that are on record, M. Fredet observes that they may be divided into two categories as regards treatment. In the first, the affection is so slight as hardly to attract the patient's attention, and then silence and rest suffice. In the more complicated cases, the patient may induce death before any treatment can be put into force; or they may be accompanied by dyspnœa, cyanosis, convulsions, aphonia, tumefaction, or deformity of the neck, emphysema, &c.; and as death may be produced at any moment by displacement of the fragments, or by œdema of the glottis, the surgeon should, even when the symptoms are not very urgent, at once practice catheterism of the larynx, or, better still, tracheotomy. Had this been done in the case related above, the patient's life would probably have been saved.—*Gaz. des Hôp.*, Nos. 90 and 91.

Case of Rupture of Anchylosis.—M. Sonrier relates a case which is of considerable interest, both as regards its origin and termination. A sergeant, æt. 45, entered the camp hospital at Chalons in May, 1867, to be treated for stricture of the urethra; and during the process of gradual dilatation he was seized with violent arthritis of the knee (never having suffered from rheumatism or met with any accident), which lasted a week, in spite of active antiphlogistic treatment. Suppuration ensued, and matter was evacuated by incisions in the middle of July, the amount which was discharged being enormous. By the beginning of October complete anchylosis, both extra- and intra-capsular, had formed. In the middle of that month the first attempt at rupturing the anchylosis was made, but it was not until after seven *séances*, at intervals of from five to fourteen days, that this could be completely accomplished. On two occasions a terrible amount of inflammation was reproduced. This, however, subsided, and by the 18th January the soldier was enabled to join his corps. A year after the accident he was able to walk two or three leagues without fatigue.—*Ibid.*, No. 80.

On the Conservative Treatment of Gun-shot Fractures of the Femur.—In a letter addressed to Professor Palasciano, Signor Gritti, Surgeon-in-chief of the Milan Hospital, adduces the experience of the late Italian war as corroborative of his views of the value of conservative surgery in gun-shot fracture of the femur contained in a work on that subject published in 1866. This additional material consists of 49 cases observed carefully during the war of 1866. Other cases must also have occurred of which no notes have been procurable, for as the wounded amounted to 3,619, the proportion of 2 per cent. of cases of fractured femur, exhibited by former statistics, would bring the total number of cases up to about 70. However this may be, here are the results observed in these 49 cases. In 1 the head of the femur was fractured and the patient recovered; in 5 the trochanters were fractured, with 5 recoveries; in 6 the upper third of the shaft of the bone was fractured, all recovering; in 15 its middle third, with 4 deaths and 11 recoveries; in 6 its lower third, with 5 recoveries and 1 death; in 11 cases the condyles were fractured, with 2 deaths and 9 recoveries; in 4 cases, all recovering, the seat of fracture was not specified. Thus, of the 49 cases, 41 recovered and 8 died. So favorable a result calls for some detailed account, and this the author furnishes in a tabular statement of the particulars of each case as regards its treatment. Summing this up, he reports that in 30 of these cases, in which the conservative treatment was pursued throughout, there were 2 deaths and 28 recoveries; in 12, in which conservative treatment had to be supplemented by amputation, 6 died and 6 recovered; and 7 cases, in which amputation within 24 hours was performed, all recovered. Thus, while of 42 cases treated conservatively, 28 recovered, all those did so in which immediate amputation was performed. Dr. Gritti, however, notwithstanding the remarkable success attending this operation, believes that it should be confined within narrow and precise limits, and that its success on this occasion was much owing to the exceptional circumstances under which it was performed. The patients were treated in small hospitals, in a healthy and mountainous locality, and were furnished with a nutritious diet. The 42 cases treated conservatively, with and without consecutive amputation, furnished a minimum mortality of 19·04 per cent.; and a more intelligent application of the principles of this treatment will be followed by still more favorable results. The dispersion of patients which acted so beneficially in the Austrian and Prussian armies was not easy of accomplishment in Italy, where the patients requiring prolonged treatment were consigned to overcrowded hospitals situated in the centres of towns, and often fell victims to typhus or pyæmia. The extraction of foreign bodies was not performed so effectually as it ought to have been, while the means of transport of the wounded were defective.

After adverting to some of the cases in particular, Signor Gritti concludes by saying that the clinical facts observed during this war confirm the value of the conservative treatment of fracture of the femur, especially when this takes place at its upper port, which is just the contrary to what happens in amputation, this failing more and more the higher up the limb it is employed at. But in order to succeed in conservative surgery the surgeon must be thoroughly indoctrinated with its principles. His exploration of the wound also ought to be prompt and complete, so that he may at once decide whether conservative or demolitionary treatment should be resorted to. If the first is decided upon he should remove from the wound, making dilating incisions for the purpose if necessary, all foreign bodies and splinters; and then place the limb in an apparatus which at once secures its immobility and reduction, and allows of easy access to the wound. Had the surgeons employed in 1866 more exactly fulfilled these conditions they probably would have saved some lives by performing immediate amputation, while, in other cases, they might have avoided the too tardy extraction of foreign bodies, the development of phlegmonous and profuse suppuration, and perhaps some of the consecutive amputations.—*Annali Universali di Medicina*, September.

Orchichorea.—Under this fanciful title Dr. Löwer describes a condition of the testis which he says he has met with several times, but only in the subjects of onanism. In this the testes, ordinarily at rest in the scrotum, are one or both in constant motion, and this motion exhibits itself under three varieties. In one of these, which the author terms "orchichorea alterna," each testis is alternately rising and sinking in the scrotum; in another, "completa," both testes are rising and sinking at the same time; and in the third, "simplex," only one testis is so occupied. He has met with the first of these eight times, with the second five times, and with the third three times. The movements are quite painless, and continue unchanged whatever may be the position of the body, and are not interrupted by manipulation. These varieties indicate the different degrees at which the manustapration has reached, the unilateral form attending the slightest of these and the "orchichorea completa" the most advanced degree. The affection, which is truly a pathognomonic sign of onanism, only admits of gradual relief on relinquishing the habits which have given rise to it.—*Deutsche Klinik*, 15th August.

Additional Account of the Losses sustained by the Italian Army in the Campaign of 1866.—Professor Cortese, formerly Medical Director of the Italian Army, publishes this paper as a supplement to a document which he has already inserted in the 'Transactions of the Venetian Institute of Science.'

He observes that this campaign, by reason of its short duration, its being fought on a friendly soil, at a fine season of the year, with a good supply of provisions and all necessaries, and with an exemption from all kinds of privations, offers little for remark except as regards the traumatic lesions that were produced. The cholera, indeed, was the only disease of any importance that was met with; and the author agrees with Heyfelder, reporting on the Prussian campaign of the same year, that ample evidence of the importation of this scourge exists. The total number of wounded in the Italian army amounted to 3735, viz. 2903 among the regular troops, and 832 among the volunteers. Of the 2903 wounded in the regular army, 239 were officers, or 8·23 per cent.; in the volunteer army 32, or 3·85 per cent. In the campaign of 1859 the proportion of officers wounded in a total of 4,689 was 4·94 per cent. In the Prussian campaign of 1866 there were among the 16,177 (not counting those who died on the field of battle) wounded 669 officers, or 3·10 per cent. In the Italian campaign of 1866 there died either on the field of battle, or immediately afterwards, 651, and therefore a proportion of 1 to 4·77 of the wounded. In 1859 this proportion was 1 to 4·88; and in the Prussian army the deaths were 2931, or 1 to 5·50 wounded. The mortality of the Italian armies was, however, still greater than stated above, for during the two years which have elapsed since the end of the campaign 670 of the wounded have perished, 314 in the Italian and 356 in the Austrian hospitals.

Professor Cortese congratulates himself at finding precepts which he had laid down in 1859 so amply sanctioned by the results of the late Prussian campaign, as compared with those obtained in the Danish war. The Prussians found their hospitals well supplied with all requisites and ample assistance; and to prevent the effects of agglomeration supplemented them by tents and barracks. Simple and conservative treatment was also found far superior to mere heroic procedures, especially when aided by good diet. A great check was thus given to the prevalence of pyæmia. The experience of the Italian campaign has been similar. The hospitals were in an admirable condition, and well supplied with assistance, and those of the Austrians, which fell into their hands, were found to be, as were those which the Prussians gained possession of, in admirable order, clean, and in good sanitary condition. The number of wounded taken by the ambulances to the Italian hospitals was 2811, while 737 remained prisoners with the Austrians, and 187 were found in the Venetian hospitals, making a total of 3735. Of those that died in the Italian hospitals 314, and in the Austrian 356, or a total of 670. The total mortality of the wounded was thus,— 1 : 5·58, or 1 : 8·95 in the Italian, and 1 : 2·59 in the Austrian hospitals. This latter greater mortality arose in part from the greater gravity of the wounds, and in part from the great distances over which the wounded prisoners had to be transported.

The results are examined by the author, after distributing the cases into four groups, according to the regions of the body implicated. Three hundred cases had to be left out, owing to defective indications, but the remaining 2603 were thus distributed,—1st group, implicating the head, face, and neck, 361 cases, or 13·86 per cent.; 2nd group, thorax, abdomen, back, pelvis, and shoulders, 550, or 21·13 per cent.; 3d group, the upper extremity, 785, or 30·16 per cent.; 4th group, the lower extremity, 907, or 34·85 per cent. This excess of wounds of the extremities over those of the head and trunk is constantly observed; but in the present campaign the wounds of the hand were especially numerous (378); those of the face, too (163), were also very numerous when it is considered how small is the surface exposed. Among the pensioned soldiers, 122 were found to have undergone amputations in the following proportion,— at the shoulder-joint, 7; of the humerus, 39; of the forearm, 10; of the hand and fingers, 12; at the hip-joint, 22; of the leg, 28; partial of the foot, 2; excision of the head of the humerus, 2. Several of these amputations were performed in the Austrian hospitals; and the author is of opinion that some of the lesions, especially those of the arm, might have been treated on conservative principles.—*Annali Universali di Medicina*, September.

Reduction of Dislocation of the Shoulder by the Pendulum Method.—Dr. Uterhart in this paper relates five additional cases occurring at the Rostock Surgical Clinic, as illustrating Professor Simon's new mode of reducing this dislocation. We have described this in our number for January, 1867, p. 269, and need only observe here that the chief feature of the procedure consists in raising the patient's body by the luxated limb, while this is manipulated into its cavity. In the first case the dislocation had existed twenty-four hours, and had resisted the ordinary means, but quickly yielded to combined elevation and pressure. In two others, in each of which the dislocation had taken place eight hours before, reduction was quickly accomplished, chloroform being employed on account of the sensitiveness of the parts. One of these patients was seventy-two years of age. In the fourth case the dislocation had existed for forty-eight hours, and had resisted various forcible endeavours. The patient was raised from the floor by means of a pulley attached to the arm, and the reduction took place at once. In the fifth case the injection of morphia, recently recommended as a substitute for chloroform, was tried, but without effect; but when the patient had been chloroformised, scarcely was his body raised from the floor by the pulleys than the reduction was accomplished. These and various other cases testify to the efficacy of this method; and to the objection that it is a violent one, it may be replied that the weight of the body is only brought gradually into operation as the agent of extension. Chloroform powerfully aids the reduction, a few drams sufficing to produce the necessary relaxation. In drunkards, however, it causes dangerous excitement, and in them the morphia injection might be substituted.—*Berliner Klinische Wochenschrift*, August 10.

SUMMARY.

Amputation.—Guyon. Supra-malleolar Amputation by a New Procedure. (Gaz. des Hôp., Nov. 7.)—Volkmann. Three Cases of Amputation at the Hip-joint. (Deutsche Klinik, Oct. 17 and 24.)

Aneurism.—Ciniselli. Case of Aneurism of the Ascending Aorta, successfully treated by Galvano-Puncture. (Gaz. des Hôp., Nov. 17. Patient at the end of seventy-four days resumed his employment as a coachman.)

Breast.—Sevestre. Case of Cystic Tumour of the Breast, with Papillary Vegetations. (Gaz. des Hôp., Oct. 6. Woodcut.)

Dislocations.—Streubel. Treatment of Old Dislocations. (Prag. Viertel., 1867, B. 4. Elaborate Clinical Observations on sixteen cases.)—Hassan Mahmoud. Dislocation of the Wrist. (Gaz. des Hôp., Sept. 17. Under Desormeaux, at the Necker.)

Ear.—Pomeroy. Tinnitus Aurium. (New York Medical Journal, Sept.)—Gottstein. Mechanical Dilatation of the Meatus in Acute External Otitis. (Berlin Klin. Woch., Oct. 26. States that great advantage results from compression of the walls by means of tents of compressed sponge during the suppurative stage.)

Excision.—Doutrelepont. Regeneration of Bone after Subperiosteal Resection of Joints. (Arch. für Klin. Chirurgie, B. ix, H. 3.)

Eye.—Jeffries. Section of the Optic and Ciliary Nerves in Sympathetic Ophthalmia. (Boston Med. and Surg. Journ., Sept. 3.)—Niemetschek. Colour Blindness. (Prag. Viertel., 1868, B. iv.)—Talko. Case of Rupture of the Choroid. (Monatsbl. für Augenh., Sep.)—Schidloff Conjunctivitis Membranacea. (Allg. Wien. Med. Zeit., Aug. 4.)—Chennevis. Case of Spontaneous Luxation of the Crystalline. (Gaz. des Hôp., Nov. 12. The lens spontaneously discharged, without obvious cause, from its capsule in each eye, at an interval of a year.)—Junge. Hyperæsthesia of the Ciliary Muscle. (Monatsb. f. Angenh., August.)—Fano. Obstinate Case of Nystagmus, cured by Myotomy. (Union Méd., Sept. 15.)—Albert Graefe. Treatment of Diseases of the Lachrymal Passages. (Monatsb. f. Augenh., Aug.)—Warlomont. Treatment of Coarctations of the Lachrymal Passages. (Presse Med. Belge, Oct. 25. Notes of eight cases, confirmatory of Stilling's treatment by incision.)

Fracture.—Pelikan. New Apparatus for Fracture of the Patella. (Deutsche Klin., July 25. The author states that he has found this, which much resembles the boiler of a locomotive, completely successful in securing adaptation.)—Bottini. Clinical Observations on the Treatment of Fracture. (Annali Univ. di Med., Oct. Based on 193 cases treated at the Novara Hospital.)—Letenneur. Case of Fracture of the Os Pubis, through Muscular Contraction. (Gaz. des Hôp., Nov. 7.)

Heart.—G. Fischer. Wounds of the Heart and Pericardium. (Arch. f. Klin. Chirurgie, B. ix, H. 3. Elaborate analysis of 452 recorded cases, occupying more than 300 pages. Brief abstracts given of all the cases.)

Hernia.—Howard. Description of an Apparatus for Teaching the Anatomy and Mechanism of Hernia. (New York Med. Journ., Oct. Diagrams.)

Hip-joint.—Discussion on Diseases of the, at the Société de Chirurgie de Paris. (Union Méd., Oct. 24, Nov. 7 and 21.)—Good and Eve. Results of Amputation and Excision of the Hip-joint in Gunshot Wounds. (Gaz. Hebd., Oct. 16. These statistics, derived from the Confederate Army, relate to twenty cases of amputation, with four recoveries, and thirteen cases of excision, with five recoveries.)

Hydrocele.—Wenz. Treatment of Hydrocele by Drainage. (Allg. Wien. Med. Zeit., Sept. 8 and Oct. 20. Six cases confirmatory of Professor Patubran's good opinion of the procedure.)

Jaw.—Demarquay. Retraction of the Tongue during Removal of the Lower Jaw. (Bull. de Thérap., Sept. 15 and 30.)

Lamp.—Ravoth. New Operation Lamp. (Berlin Klin. Woch., Sept. 7. Describes and figures what seems likely to be a highly useful lamp, which, with mirror attached, may be bought for six or seven shillings.)

Laryngoscope.—De Cristoforis. New Laryngoscope. (Annali Uni. di Med., Oct. Woodcuts.)—Oliver. New Laryngoscopic Lantern, or Light-Concentrator. (Boston Med. and Surg. Journ., Oct. 8. Woodcuts.)

Larynx.—Henocque. Report on Traumatic Fractures of the Larynx. (Gaz. Hebd., Sept. 25 and Oct. 2.)

Ovariotomy.—Boinet. Successful Case of Ovariotomy in a subject of Umbilical Hernia and Prolapsus Uteri. (Union Méd., Nov. 21.)—Demarquay. Large Cystic Tumour of the Uterus mistaken for an Ovarian Cyst.) (Ibid., Sept. 22 and 26. Partial ablation of the uterus, with fatal result. Woodcut.)—Heydloff. Successful Case. (Berlin Klin. Woch., Aug. 3.)—A. Wagner. Four Cases. (Ibid., Sept. 28 and Oct. 5 and 19. One in which the ligature was cut short successful; the others died.)—Stilling. Successful Case by the Extra-peritoneal Method. (Deutsche Klin., Aug. 8.)—Neugebauer. Successful Case. (Monats. für Geburtsk., Sept. Occurred at Warsaw, and reported in great detail.)—Liegeois. Successful Case. (Gaz. des Hôp., Nov. 21. Patient fifty-four years of age, with affection of the bladder. The tumour weighed twenty pounds, but unattended with complications, and the operation was performed in the country.)—Gallevand. Clinical Lecture on a Fatal Case. (Arch. de Med. Navale, Aug. and Oct. Delivered at the Brest Naval School.)

Plastic Surgery.—Péan. Autoplasty of the Neck, the flap derived from the throracic region. (Bull de Thérap., Oct. 15. Illustrations.)—Whitehead. New and Successful Operation for Cleft Hard Palate. (Amer. Journ. Med. Sci., October. Analysis of 55 other cases and numerous illustrations.)—Schneider. Plastic Operation applied to the case of Ulcer of the Leg. (Archiv f. Klin. Chir., B ix, H. 3. Obstinate ulcer in a lad æt. 17 successfully treated. The same treatment suggested for ulcer due to conical stump after amputation.)

Scapula.—Case of Excision of the Entire Scapula. (American Journ. Med. Sci., October. Performed on a child 6 years of age. Tabular view given of 55 other cases, in which the whole or great part of the scapula has been removed. Illustrations.)

Spine.—Engel. Curvature of the Spine in its Anatomical Relations. (Wien. Med. Woch., August 15 and 22.)—Elliott. Caries of the Spine. (Dublin Journ., November. Exemplifying the utility of a leathern support, contrived by the author. Illustrations.)

Syphilis.—Verneuil and Fournier. Dropsy of the Tendinous Sheaths of the Extensors in Secondary Syphilis. (Gazette Hebd., September 25 and October 9.)

Testis.—Birch-Hirschfeld. Cancer of the Testis. (Archiv f. Heilkunde, 1868, No. 6. Describes minutely the anatomical and histological characters. Engravings.)

Tracheotomy.—Isambert. Employment of a modified form of Langenbeck's Dilating Tenaculum. (Union Méd., October 1 and 6. Woodcuts.)

Tumours.—Croly. Cases of Large Tumours successfully removed. (Dublin Journ., November. Numerous Illustrations.)

Urinary Organs.—Langlebert. New Urethroscope. (Gaz. des Hôp., October 3. Woodcut.) Thorp. Rupture of the Urinary Bladder. (Dublin Journ., November. Remarkable Case of Recovery.) Bumstead. Treatment of Stricture. (American Journ. Med. Sci., October. Passes in review the various instruments employed, approving of Holt's instrument, of which he states he has contrived a useful modification. Woodcuts.)—Morpain. Employment of Amussat's Double Lithotome. (Bull. de Thérap., September 15. Two cases given to show its utility in the removal of large calculi. Scriven. Indian Experience in Lithotrity. (Indian Medical Gazette, August. Speaks highly of it from his experience in 36 cases; and believes that with the improved instruments now in use it will make more rapid advance in India, the natives being favorable to it.)

Vagina.—Lücke. Operation for Atresia Vaginæ, terminating fatally. (Berlin Klin. Woch., July 20.)

Whitlow.—Hamilton. On Whitlow. (Dublin Journ., November. Observations founded on a severe form of the disease recently prevalent in Dublin. Coloured Illustrations.)

CONTRIBUTIONS TO MEDICAL LITERARY HISTORY.

ADVERSARIA MEDICO-PHILOLOGICA.

BY W. A. GREENHILL, M.D. OXON.

PART VI.

(*Continued from* vol. xxxviii, p. 560.)

[IN resuming these "Adversaria" I would again request those persons who may consult this collection of words to bear in mind that it does not pretend to completeness in any way. It is very far from being a complete collection of Greek medical terms, for not only have all botanical and chemical words been designedly omitted, but also those belonging especially to the Hippocratic Collection, which are sufficiently explained in the excellent 'Œconomia Hippocratica' of Foës; neither is the treatment of each word to be considered complete, for doubtless various meanings are passed over, and probably better passages might frequently have been quoted. It is simply a contribution (and that a very imperfect one,) to Greek medical technology.]

βαδιστικός, *fitted for walking*. Galen calls the leg *βαδιστικὸν ὄργανον*, and says that it is especially suited to man as being *βαδιστικὸν ζῶον*.[1]

βαλανεῖον, like *balneum* in Latin, is used in three senses, and signifies—either, 1, a *place for bathing*;[2] or, 2, a *bath in general*;[3] or, 3, especially *a hot bath*, viz. either the *laconicum and caldarium* together,[4] or perhaps the *laconicum* alone.[5] See Dr. Daremberg's note to Oribasius, tome ii, pp. 881, 882, from which this article is abridged.

βάλανος, *an acorn* (Lat. *glans*). Hence (omitting its botanical meanings,[6]) applied, from similarity of shape, to the extremity of the penis. Aristotle mentions it as the ordinary name in his time,[7] and it continued to be the most usual, if not the only, name used by the Greeks[8] down to the middle of the sixteenth century.[9] Meletius, a writer of the seventh or eighth century, says[1] that it means the *penis;* but this is probably a mistake. The Latin synonym, *glans,* was early employed in the same sense, and continues in use to the present day.[2]—In the Hippocratic Collection *βάλανος* is used both for a *suppository* and a *pessary;*[3] in later Greek writers probably only in the former sense.[4] Hence *βαλανίζω,*[5] *to administer a suppository* (or *a pessary?*), and *βαλανισμός*, used by Cælius Aurelianus[6] to signify 'pessulus,' a *pessary* (or perhaps *a suppository*). The diminutive forms *βαλάνιον*[7] and *βαλανίς*[8] are also found.

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[1] 'De Usu Part.,' iii, 4, tom. iii, p. 184, l. 10; c. 5, p. 190, l. 11. These two passages are copied by Theophilus, 'De Corp. Hum. Fabr.,' p. 41, l. 1; p. 42, l. 11, ed. Oxon.

[2] Galen, 'De Meth. Med.,' x, 10, tom. x, p. 715, l. 9.

[3] Id., *ibid.*, p. 722, l. 14.

[4] Id., *ibid.*, p. 717, l. 12.

[5] Id., 'De Sanit. Tu.,' ii, 4, tom. vi, p. 185, l. 4.

[6] See a note by Sprengel on Dioscorides, 'De Mat. Med.,' i, 145, tom. ii, p. 407.

[7] 'Hist. Anim.,' i, 10 (13), 3; copied by the anonymous author of the 'Introd. Anat.,' cap. 28.

[8] See Rufus Ephesius, 'De Corp. Hum. Part. Appell.,' p. 31, l. 14, ed. Clinch; Aretæus, 'De Cur. Morb. Acut.,' i, 1, p. 140, l. 1, ed. Adams; Galen, 'De Meth. Med.,' v, 15, tom. x, p. 381, ll. 10, 14; Pseudo-Galen, 'Introd.,' c. 10, tom. xiv, p. 706, l. 4; c. 19, p. 787, l. 8; Paulus Ægineta, vi, 53, pp. 236, 238, ed. Briau.

[9] Hypatus (*i. e. Georgius Sanguinatius*), 'De Corp. Part.,' p. 154, ed. Bernard.

βαλάντιον, the scrotum, a popular word, distinguished from the scientific term *ὄσχεος.*[9] Compare *bursa* in Latin, *bourse* in French, and *purse* in English.

βαλαντοειδὴς (or, more correctly, *βαλαντιοειδὴς*) *χιτὼν* signifies the *tunica vaginalis,* being synonymous with *ἐρυτρός*, which was another name for *ἐρυτροειδὴς* (or *ἐλυτροειδὴς*) *χιτών.*[10]

βαλμός, a comparatively modern term, signifying the *lung,*[11] or the *chest* (*στῆθος*) in general.[12] Dr. Daremberg conjectures that the word may be a corruption of the Latin *pulmo.*

βαρυηκοέω, to be hard of hearing.[13]

βαρυηκοΐα, hardness of hearing.[14]

βαρυήκοος, hard of hearing, deaf;[15] also *deafening,* applied to the south wind (*νότος*).[16]

βασιλικός, royal, an epithet "applied to parts and medicines from their importance or presumed excellence." (Mayne.) Perhaps Euelpides (a celebrated oculist in the time of Celsus[17]) was the first who did this when he gave the name to a collyrium.[18] It was also given to the ointment called *τετραφάρμακον,*[19] and to other compounds besides.[20] The "Basilicon Ointment" of modern pharmaceutical works differs from the ancient compound.

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[1] 'De Nat. Hom.,' p. 112, l. 15, ed. Cramer.

[2] Celsus, 'De Medic.,' vi, 18, 4; vii, 25, 1, pp. 257, 305, ed. Daremb.

[3] See Foës, 'Œcon. Hippocr.'

[4] Rufus Ephesius, quoted by Oribasius, 'Coll. Med.,' viii, 39, tome ii, p. 254, ed. Daremb.; Paulus Ægineta, i, 45, p. 6, ed. Ald.

[5] Hippocrates, quoted by Julius Pollux, 'Onomast.,' x, 31, § 150.

[6] 'Morb. Chron.,' vi, 7, § 105, p. 533, ed. Amman.

[7] Oribasius and Paulus Ægineta, *locis cit.* In Oribasius, p. 255, l. 12, instead of *ταῖς ἀναδάκνουσιν ἰσχυρῶς βαλάνοις*, we should read *τοῖς ἀναδάκνουσιν ἰσχυρῶς βαλανίοις*, as may be seen from the Var. Lect.

[8] Hippocrates, 'De Morb. Mul.,' ii, 155, tome viii, p. 330, ed. Littré.

[9] Theophilus, 'De Corp. Hum. Fabr.,' v, 28, p. 225, l. 7, ed. Oxon.

[10] Meletius, 'De Hom. Fabr.,' p. 115, l. 4, ed. Cramer.

[11] Sanguinatius, in Daremberg's 'Manuscrits Médicaux Grecs d'Angleterre,' p. 132, n. 39.

[12] Hesychius and Suidas.

[13] Hippocrates, 'De Morb.,' ii, 4, tome vii, p. 10, l. antep.

[14] Id., 'Aphor.,' iii, 17, 31, tome iv, p. 494, l. 5; p. 502, l. 2.

[15] Aretæus, 'Caus. Morb. Chron.,' i, 4, p. 55, l. 18, ed. Adams.

[16] Id., 'Cur. Morb. Chron.,' i, 3, p. 214, l. 6; Hippocrates, 'Aphor.,' iii, 5, tome iv, p. 488, l. 1, ed. Littré; 'De Humor.,' § 14, tome v, p. 496, l. 1.

[17] Celsus, 'De Medic.,' vi, 6, § 8.

[18] Id., *ibid.*, vi, 6, § 31.

[19] Galen, 'De Compos. Medic. sec. Loc.,' iii, 1, tom. xii, p. 601, l. ult.

[20] See Galen, tom. xii, p. 782, l. 6; xiii, 184, 6; xiv, 174, ult.

The case of the *vena basilica* is very singular, as the earliest writers (so far as is at present known) in whose works the name is found are not the Greek, but the Arabic;[1] and it is made still more puzzling by the fact that the Greek translator, (who is supposed to have lived in the eleventh century,) when he meets with the word باسليق in the Arabic, does not use the corresponding Greek word βασιλική, but calls the vein in question ἡ μέση φλέψ.[2] The only Greek work in which I have found the word is the Pseudo-Synesius, 'De Febribus,'[3] which is, in fact, a translation from the Arabic work of Abú Jaáfar Ahmed, by an unknown writer.[4] Perhaps the earliest Latin writer in whose works the word is found is Constantinus Afer, who lived in the eleventh century, and translated this same Arabic treatise into Latin under the title 'Viaticum.'[5]

βελονοειδής, *needle-shaped*, applied (perhaps exclusively) to the styloid process of the temporal bone, which is generally called ἀπόφυσις,[6] but sometimes ἔκφυσις.[7]

βιασμός, a popular name for τεινεσμός, or *inflammation of the rectum*.[8]

βλαρία, a popular name for ἄση, or *nausea*.[9]

βλεφαρικός, *relating to the eyelids*, applied to collyria.[10]

βλεφαρίς means sometimes *an eyelid*,[11] but more commonly (in the plural) *the eyelashes*.[12]

βλέφαρον, *an eyelid*. The diseases of the eyelids are enumerated by Galen.[13]

βλεφαρόξυστον, an instrument for removing callosities from the inner surface of the eyelid.[14]

βλεφαροτόμον, a scalpel for the eyelids.[15]

βοθρίον, *a little pit*, is used to signify—1, the *alveolus*, or *socket of a tooth*, or, more precisely, the cavity in which the tooth is inserted, the walls of the cavity being called φατνίον;[16]—2, *an ulceration of the*

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[1] Rhazes, 'De Var.,' p. 82, l. 2, ed. Channing. See note K, in the English translation, p. 152.

[2] P. 250, l. 13 from bottom. In Paulus Ægineta ἡ μέση φλέψ signifies, not the *basilic*, but the *median* vein (vi, 40, p. 190, ll. 12, 16, ed. Briau).

[3] P. 278, l. 4.

[4] See Dr. Daremberg's 'MSS. Médicaux Grecs d'Angleterre,' p. 63, &c.

[5] P. 41 l. 16, at the end of Bernard's ed. of Synesius, 'De Febribus.'

[6] See ἀπόφυσις βελονοειδής in Part IV.

[7] Galen, 'De Musc. Dissect.,' tom. xviii B, p. 957, l. 11; p. 959, l 9; copied by Oribasius, 'Coll. Medic.,' xxv, 36, 37, tome iii, p. 444, l. 11; p. 445, l. 9.

[8] Leo, 'Consp. Medic.,' v, 12, p. 176, in Ermerins, 'Anecd. Med. Gr.'

[9] Id., *ibid.*, v, 5, p. 171.

[10] Cælius Aurelianus, 'Morb. Chron.,' iv, 2, § 17, p. 498, ed. Amman.

[11] Aristotle, 'Hist. Anim.,' i, 8, § 2; 'Part. Anim.,' ii, 13, § 1, ed. Tauchn.

[12] Id., *ibid.*; Rufus Ephesius, p. 24, l. 11; p. 48, l. 10, ed. Clinch.

[13] 'Defin. Med.,' c. 351, tom. xix, p. 436.

[14] Paulus Ægin., iii, 22, p. 34, l. 34, ed. Ald.

[15] Leo, 'Consp. Med.,' iv, 5, p. 155, in Ermerins, 'Anecd. Med. Gr.'

[16] Galen, 'De Oss.,' c. 5, tom. ii, p. 754, ll. 10, 11, copied by Oribasius, 'Coll. Medic.,' xxv, 6, tome iii, p. 401, ult.; p. 402, l. 1.

cornea, distinguished from *κόιλωμα* as being deeper and narrower.[1] In this latter sense it is found in the Hippocratic Collection,[2] and explained by Erotianus.[3]

βόμβος, a word in the Hippocratic Collection to express *a buzzing sound in the ears, tinnitus aurium;*[4] also, in later writers, a *rumbling in the intestines*,[5] differing slightly from *βορβορυγμός* as arising more entirely from flatus.

βομβώδης, *humming, buzzing.*[6]

βορβορυγρός, *the noise caused by flatus in the intestines with a small amount of fluid*, in which sense the word is used in the Hippocratic Collection.[7] Leo apparently says[8] that Galen uses the words *ἐλκύδριον* and *κλύδων* in the same sense. The former word I have never found in this sense, but *κλύδων* is used to signify *fluctuation* in 'De Sympt. Caus.,' iii, 2, tom. vii, p. 215, ll. 8, 10, 13, which may therefore, perhaps, be one of the passages referred to by Leo.

βούβων, a word as old as Homer, by whom it is used[9] (no doubt) in the usual anatomical sense of *the groin*. It occurs frequently in the Hippocratic Collection, where it signifies sometimes *the groin*, sometimes *the inguinal glands*, sometimes *inflammation of the glands*, whether of the axilla or the groin or the neck.[10] In the common anatomical sense it is used constantly by later writers, generally, perhaps, in the singular number,[11] but also in the plural.[12] The supposed derivations of the word, either "from Heb. *bobo*, by reduplication of בוא *bo*, to enter; or, *βοῦ*, a particle of increase, and *βαίνω*, to run into," scarcely require refutation, though the latter conjecture is as old as the time of Meletius, probably in the seventh or eighth century after Christ.[13]

The most interesting circumstance connected with the word is its relation to the oriental, or glandular, or true Plague, which till recently was supposed to have been unknown in Europe before the time of Justinian (the sixth century), and to have been first described by Procopius, the historian. In 1831 Cardinal Mai published a fragment of Rufus Ephesius,[1] which was first prominently brought into notice in 1840 by M. Littré,[2] and which proves conclusively that the buboes occurring in the groins and armpits in the course of a pestilential fever were known at least as early as the first century after Christ. This being the case, it has been supposed with some probability that in some passages in the Hippocratic Collection *βούβων* signifies the *bubo* of the true Plague,[3] and Adams[4] even considered that "the pestilence which prevailed [in Athens] during the Peloponnesian war partook of the nature of the glandular plague," though there is no mention of *buboes* in the account given by Thucydides.[5] This, however, though quite possible, can hardly be considered probable, as it is inconceivable that so important a symptom should (if it had really been present) have been entirely overlooked by so accurate a writer as Thucydides, even though he was not himself a physician.

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[1] Paulus Ægin., iii, 22, p. 34 B, l. 23, ed. Ald.; Joannes Actuar., 'De Morb. Diagn.,' ii, 7, in Ideler's 'Phys. et Med. Gr. Min.,' vol. ii, p. 446, l. 22. See also Galen, 'Introd.,' c. 16, tom. xiv, p. 774, l. 7; 'Defin. Med.,' c. 333, tom. xix, p. 434; Leo, 'Consp. Med.,' iii, 30, p. 145, in Ermerins, 'Anecd. Med. Gr.'

[2] 'De Liquid. Usu,' § 6, tome vi, p. 132, l. 13, ed. Littré.

[3] 'Voc. Hippocr. Coll.,' p. 58, ed. Klein.

[4] 'Coac. Prænot.,' § 189, tome v, p. 624, ed. Littré.

[5] Galen, 'De Sympt. Caus.,' iii, 6, tom. vii, p. 241, l. ult.; Cælius Aurel., 'Morb. Acut.,' ii, 32, § 169, p. 149, ed. Amman.

[6] Galen, *ibid.*, p. 241, l. 12.

[7] 'Prognost.,' § 11, tome ii, p. 138, l. 11, ed. Littré. See Foës, 'Œcon. Hippocr.'

[8] 'Consp. Medic.,' v, 15, p. 179, in Ermerins, 'Anecd. Med. Gr.'

[9] 'Il.,' iv, 492.

[10] See Foës, 'Œcon. Hippocr.;' Index to Kühn's Galen.

[11] Galen, 'De Anat. Admin.,' iii, 10, tom. ii, p. 398, l. 7; p. 399, l. 1, and elsewhere.

[12] Id., *ibid.*, vi, 13, p. 583, l. 1; Rufus Ephes., p. 33, l. 1, ed. Clinch; Theophilus, 'De Corp. Hum. Fabr.,' i, 23, § 4, p. 52, l. 7, ed. Oxon., and elsewhere.

[13] 'De Hom. Fabr.,' c. 24, p. 110, l. 2, in Cramer's 'Anecd. Gr.,' vol. iii.

βουβωνοκήλη, inguinal hernia, is thus distinguished by Paulus Ægineta[6] from *ἐντεροκήλη* :—"These diseases differ in situation only; for, when the intestine remains in the groin, it is called *βουβωνοκήλη*, and when it falls down into the scrotum, it is named *ἐντεροκήλη*." Perhaps Celsus is the earliest extant writer who uses the word,[7] which is found also in Galen[8] and Leo.[9]

βουβωνοκηλικός, relating to inguinal hernia.[10]

βούλιμος, a voracious appetite. Perhaps Erasistratus is the earliest medical author who is known to have used the word,[11] which is frequently met with in the Greek writers,[12] and is one of the medical words adopted by the Arabians, and used in the ordinary sense.[13] Leo explains the word to mean *an entire want of appetite*,[14] but I have not noticed it used in this sense elsewhere.

The verb *βουλιμιάω, to have a voracious appetite*, was also used by Erasistratus,[15] and is found in Galen,[16] and elsewhere.

The adjective, *having a voracious appetite*, is found in the form *βουλιμιώδης* in Kühn's edition of Galen;[17] Liddell and Scott in their

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[1] 'Classici Auctores e Codicibus Vaticanis editi,' vol. iv, p. 11.

[2] In the second vol. of his Hippocrates, p. 584.

[3] See M. Littré's Hippocrates, tome iii, p. 1, &c.; and M. Daremberg, 'Œuvres d'Hippocr.,' p. 590, ed. 2nd.

[4] Hippocrates, p. 384, ed. Syd. Soc.

[5] ii, 47, &c.

[6] iii, 53, vol. i, p. 589, ed Syd. Soc. See also vi, 66, vol. ii, p. 377.

[7] vii, 18, p. 297, ed. Daremberg.

[8] 'De Tumor. præter Nat.,' c. 16, tom. vii, p. 730, l. 11.

[9] 'Consp. Medic.,' vi, 10, in Ermerins, 'Anecd. Med. Gr.,' p. 195.

[10] Paulus Ægin., vi, 66, p. 282, l. pen.; p. 286, l. 9, ed. Briau.

[11] In Aulus Gellius, 'Noct. Att.,' xvi, 3.

[12] See Index to H. Stephani 'Medicæ Artis Principes,' and to Kühn's ed. of Galen; also Foës, 'Œcon. Hippocr.'

[13] Avicenna, vol. i, p. 440, l. 15, ed. Arab.

[14] 'Consp. Medic.,' v, 2, p. 169, in Ermerins, 'Anecd. Med. Gr.'

[15] *Loco cit.*

[16] 'De Puls. ad Tir.,' c. ult., tom. viii, p. 490, l. 7, copied by Theophilus, 'De Puls.,' p. 75, l. 1, in Ermerins, 'Anecd. Med. Gr.'

[17] 'De Comp. Medic. sec. Loc.,' viii, 1, tom. xiii, p. 122, l. 5.

Lexicon only mention the form *βουλιμώδης*, but as there is the substantive *βουλιμία*, as well as *βούλιμος*, probably both forms of the adjective are admissible.

βούπεινα, a word used by Erasistratus to signify *a voracious appetite.*[1]

βράγχος sometimes signified simple *hoarseness*, distinguished from *ἀφωνία*, or complete *loss of voice*, by being less in degree, not different in kind.[2] Sometimes it was used to signify the affection from which the hoarseness proceeded, and which is thus distinguished by Paulus Ægineta[3] from *κόρυζα* and *κατάρροος*:—"All these complaints have this in common, that they are occasioned by the defluxion of a redundant humour from the head to the parts below. When, therefore, it seats in the nostrils, the complaint is called *κόρυζα*; when in the pharynx and roof of the mouth, simply *κατάρροος*; but when it attacks the larynx and arteria trachea, so as to occasion a roughness of the membrane which lines them, the voice becomes hoarse (*βραγχώδης*), and the complaint is called *βράγχος* and *ἀρτηριακὸν* [*πάθος*]."[4] Sometimes it seems to mean either the cause or the effect, or perhaps both, as in the description of the Plague of Athens by Thucydides,[5] where something more than mere *hoarseness* is probably intended, though this also may be included.[6]

βραγχώδης sometimes, in the Hippocratic Collection, means *hoarse*, as the voice;[7] sometimes, *subject to βράγχος* (probably in both senses of the word);[8] sometimes, *apt to produce βράγχος*, applied to water.[9] The word was still in use in the time of Leo.[10] The adverb *βραγχώδως* is found in Galen.[11]

βραδυπεπτέω, to have slowness of digestion.[12]

βραδυπεψία, slowness of digestion,[13] as distinguished from *ἀπεψία* and *δυσπεψία*.

βραχίων, a word found in the Homeric poems, signifying sometimes *the arm* in the common sense of the word, viz. the whole of the upper extremity; sometimes, only the upper part as far as the elbow, but including both the bone and the muscles.[1] In the Hippocratic Collection it signifies sometimes the bone itself, *os humeri*;[2] sometimes, the muscles also, from the shoulder to the elbow;[3] and it is used in the same senses indiscriminately by later writers.[4]

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[1] Quoted by Aulus Gellius, 'Noct. Att.,' xvi, 3.

[2] Galen, 'De Comp. Med. sec. Loc.,' vii, 1, tom. xiii, p. 4, l. 16, &c.

[3] iii, 28, vol. i, p. 469, ed. Syd. Soc.

[4] See also Galen, 'De Sympt. Caus.,' iii, 11, tom. vii, p. 263, l. 5, &c.; 'Comment. in Hippocr. *De Humor.*,' i, 18, tom. xvi, p. 171, l. 7, &c.

[5] ii, 49, § 2.

[6] See also Hippocrates, 'De Vet. Medic.,' § 19, tome i, p. 616, l. 12, ed. Littré.

[7] 'Epid.,' i, 1, tome ii, p. 602, l. antep.; vii, 7, tome v, p. 378, l. 9.

[8] *Ibid.*, vii, 9, tome v, p. 380, l. 7. In 'Epid.,' ii, 2, where Kühn (tom. iii, p. 443, l. antep.) and the older editions had *βραγχώδεες*, Littré (tome v, p. 98, l. 13, where there is a typographical error) and Ermerins (vol. i, p. 482, l. 2) read *βραχυμογέες*.

[9] 'De Aër. Aq. et Loc.,' § 7, tome ii, p. 26, l. 18, ed. Littré.

[10] 'Consp. Medic.,' iv, 9, p. 157, in Ermerins, 'Anecd. Med. Gr.'

[11] 'De Compos. Medic. sec. Loc.,' vii, 1, tom. xiii, p. 4, l. 8.

[12] Dioscorides, 'Mat. Med.,' v, 49, tom. i, p. 725, l. 6, ed. Sprengel; Galen, 'De Alim. Facult.,' ii, 21, tom. vi, p. 597, l. 14, copied by Oribasius, 'Coll. Medic.,' i, 49, tome i, p. 62, l. 10, ed. Daremberg.

[13] Galen, 'De Sympt. Differ.,' c. 4, tom. vii, p. 62, l. 14; p. 72, l. 1; 'De Compos. Medic. sec. Loc.,' viii, 3, tom. xiii, p. 158, l. antep.

Some persons[5] have considered that *βραχίων* signified the *forearm*, as *brachium* undoubtedly was so used by the Latin writers;[6] and, apparently, the authority of Galen has been quoted in support of this opinion. I have not had an opportunity of learning what passages in Galen's works are supposed to bear this meaning, and therefore cannot examine them; but in all the passages that I have seen in Galen and the other Greek anatomists, the word is certainly used in the sense given above, and the instances are far too numerous to be quoted.

The derivation of the word from *βραχύς*, *short*, and *κίων*, *a column* (quasi *βαχυκίων*), scarcely deserves refutation.[7]

βραχυμογής, the word adopted in one passage in the Hippocratic Collection[8] by Littré[9] and Ermerins,[10] where Kühn[11] and most former editors had *βραγχώδεες*. It is translated by Ermerins "brevi tempore laborabant,'' by Littré and Daremberg[12] "ils s'essoufflaient promptement; by Liddell and Scott the word is explained "suffering but a short time." It is not quite clear which of these is the right meaning in the passage referred to.

βραχυπνοέω, *to be subject to shortness of breath.*[13]

βραχύπνοια, *shortness of breath.*[14]

βραχύπνοος, or *βραχύπνους*, a word used in the Hippocratic Collection to signify *shortness of breath*, that is (as explained by Galen[15]), short inspirations at long intervals.[16] This restricted sense, however, is probably not always observed, and the word may be considered to signify both short and hurried inspirations.[17]

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[1] See M. Daremberg, 'La Médecine dans Homère,' 1865, p. 16, where the different passages are examined in which the word occurs.

[2] 'De Fract.,' § 8, tome iii, p. 444, ed. Littré.

[3] 'De Artic.,' § 1, tome iv, p. 80, l. 1. See Galen's Commentary on this and the last passage, tom. xviii A, p. 313; B, pp. 418, 419.

[4] See the Index to Kühn's Galen, and specially 'De Motu Musc.,' ii, 2, tom. iv, p. 427, l. 7.

[5] Van der Linden, quoted in Castelli 'Lex. Med.'

[6] Celsus, viii, 1, 10, § 3, p. 327, l. 11; p. 348, l. 14, ed. Daremberg; Tacitus, 'Germ.,' c. 17.

[7] Meletius, 'De Hom. Fabr.,' c. 27, p. 118, l. 18, in Cramer's 'Anecd. Gr.,' vol. iii.

[8] 'Epid.,' ii, 24.

[9] Tome v, p. 98, l. 13, where, by a typographical error, the word is printed *βραγυμογέες*.

[10] Vol. i, p. 482, l. 2.

[11] Tom. iii, p. 443, l. antep.

[12] 'Œuvres Choisies d'Hippocr.,' p. 637, ed. 2nd.

[13] Antyllus, quoted by Oribasius, 'Coll. Med.,' vi, 21, § 9, tome i, p. 506, l. 12, ed. Daremberg.

[14] Galen, 'De Diffic. Respir.,' iii, 10, tom. vii, p. 491, ll. 1, 8.

[15] 'Comment. in Hippocr. *Epid. III*,' iii, 74, tom. xvii A, pp. 755, 756.

[16] 'Epid.,' iii, 17, § 3, tome iii, p. 114, l. 3, ed. Littré.

[17] See Foës, 'Œcon. Hippocr.'

βραχυπότης, a word found in the Hippocratic Collection,[1] translated *parvibibulus* by Cælius Aurelianus,[2] and explained by Galen[3] (who uses the form βραχυπότος) to mean *one who drinks seldom, and little at a time,* though he also says the word is applied to those who drink more freely.[4]

βρέγμα, *the upper part of the head,* between the μέτωπον, or *forehead,* and the κορυφή, or *crown of the head,*[5] rendered by Cælius Aurelianus *medium capitis*, and *medium testæ*,[6] and answering probably to the Latin *sinciput.* The word occurs in the Homeric poems[7] and in the Hippocratic Collection,[8] and also in the later anatomists.[9] In the plural it signifies more precisely the *parietal bones,*[10] which are sometimes called "the bones of the βρέγμα,"[11] and sometimes (apparently in a more general way) the bones of the κορυφή.[12] The expression, "the bone (*sing.*) of the βρέγμα was fractured"[13] does not, of course, imply that the writer considered the βρέγμα to consist of only one bone, but rather signifies *one* of the parietal bones.—Etymologically, the word is probably connected in some way with βρέχω, *to wet,* either because it was supposed that cold and moisture continued longer in this part of the brain on account of the softer and more spongy character of this bone,[14] or because this is the part of the head to which a fomentation (ἐπιβροχή) is applied.[15]—This part of the head was also called ἁπαλόν, but this was probably a mistake, arising from the βρέγμα being said to be ἁπαλόν, *soft.* The form βρογχμός, or βροχμός, is also found in a late writer.[16]

βρογχίη is found in one place in the Hippocratic Collection,[17] where it is impossible to say in what sense the word was used by the writer, who probably had no definite idea of his own meaning. Triller thinks it means the *vena cava,*[1] others simply *a cartilaginous tube*, and Ermerins[2] has adopted the conjectural reading, *βρυχίη*.

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[1] 'Prorrhet.,' i, 16; 'Coac. Præn.,' § 95, tome v, p. 514, l. 9; p. 602, l. ult., ed. Littré.

[2] 'Morb. Acut.,' iii, 16, p. 227, ed. Amman.

[3] 'Comment. in Hippocr. *Epid. III,*' iii, 74, tom. xvii A, p. 755, l. 15.

[4] 'De Diffic. Respir.,' i, 12, tom. vii, p. 791, l. 13.

[5] Aristotle, 'Hist. Anim.,' i, 7, 8, p. 14, l. 17; p. 15, l. 2, ed. Tauchn.; Palladius, 'Comment. in Hippocr. *Epid. VI,*' in Dietz's 'Scholia in Hipp. et Gal.,' vol. ii, p. 5, l. 23.

[6] 'Morb. Acut.,' i, 11, p. 30, l. antep.; 'Morb. Chron.,' i, 4, p. 325, l. 3, ed. Amman.

[7] 'Batrach.,' v, 231. In 'Iliad,' v, 586, the form βρεχμός is used.

[8] 'De Cap. Vuln.,' 'Epid.,' vi, 1, tom. iii, p. 348, l. ult.; p. 583, l. 7, ed. Kühn. See Foës, 'Œcon. Hippocr.'

[9] Rufus Ephes.,' 'De Appell. Part. Corp. Hum.,' p. 23, l. ult.; p. 24, l. 2; p. 47, l. 13. See Index to Kühn's Galen.

[10] Galen, 'Comment. in Hippocr. *Epid. VI,*' iii, 1, tom. xvii B, p. 3, l. 14; Theophilus, 'De Corp. Hum. Fabr.,' iv, 3, 1, 9, p. 129, l. 17; p. 131, l. 16, ed. Oxon.; Meletius, 'De Hom. Fabr.,' p. 54, l. 23, in Cramer's 'Anecd. Gr.,' vol iii.

[11] Galen, 'De Oss.,' c. 1, tom. ii, p. 744, l. 14; 'De Usu Part.,' xi, 20, tom. iii p. 935, l. 14.

[12] Pseudo-Galen, 'Introd.,' c. 12, tom. xiv, p. 720, l. 12.

[13] Galen, 'De Meth. Med.,' vi, 6, tom. x, p. 452, l. 15; p. 453, l. 1.

[14] See Aristotle, 'De Part. Anim.,' ii, 7, p. 40, l. antep.; 'De Gener. Anim.,' ii, 6, p. 241, l. 18, ed. Tauchn.; Meletius, *loco cit.*, l. 3.

[15] Palladius, *loco cit.*, l. 19.

[16] See Dr. Daremberg's note on this word in his 'MSS. Méd. Grecs d'Angleterre, p. 123-4.

[17] 'De Dissect.,' tome viii, p. 538, l. 8, ed. Littré.

βρόγχιον, a diminutive of *βρόγχος*, is more commonly found in the plural, *βρόγχια*, and generally means the *cartilaginous rings of the trachea;*[3] sometimes, the *ramifications of the trachea*, or modern *bronchia,*[4] which were called also *σήραγγες* and *ἀορταί.*[5] This latter meaning is probably what is intended by Cælius Aurelianus, who speaks of the "pulmonis partes quæ arteriæ (i. e. *to the trachea*) sunt adhærentes, quas appellant *bronchia,*" and says that they were supposed by Asclepiades to be the seat of peripneumonia.[6] In one place in the Hippocratic Collection[7] it is supposed by Foës[8] to signify *the ethmoid bone*, but the writer probably had no very distinct idea of his own meaning.—The word occurs in the singular number in Galen,[9] meaning a cartilaginous ring of one of the smaller bronchial tubes, which is sometimes spit up in ulceration of the lungs; and the same expression occurs in Aretæus,[10] (where the text has been corrected by the later editors,) and in Leo, where *βρόγχον* should probably be changed into *βρόγχιον.*[11]

βρογχοκήλη does not appear to have been used by the ancient physicians to signify especially the enlargement of the thyroid gland which the moderns call *bronchocele*, or *goître*, but was applied generally to any voluminous tumour of the throat, whether the contents were solid or fluid, and even including aneurysm.[12]

βρογχοκηλικός, affected with *βρογχοκήλη.*[13]

βρόγχος, when used in a strictly anatomical sense, probably always meant *the trachea.*[14] Sometimes, however, it signified more vaguely *the throat*, as in the expression *βρόγχος ἐξέχει*, 'the throat protrudes,'[15] and the compound term *ἐξεχέβρογχος*, *having the throat prominent*.

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[1] In his Commentary on the treatise, republished in his 'Opuscula Medica,' vol. ii, p. 267.

[2] Vol. iii, p. 287, l. 13.

[3] Rufus Ephes., 'De Appell. Part. Corp. Hum.,' p. 57, l. 16, ed. Clinch; Oribasius, 'Coll. Med.,' xxiv, 11, § 10, tome iii, p. 323, l. 10, from Galen; Theophilus, 'De Corp. Hum. Fabr.,' iii, 13, §§ 1, 2, p. 106, l. pen.; p. 107, ll. 4, 6, ed. Oxon. See also Index to Kühn's Galen.

[4] Hippocrates, 'De Rat. Vict. in Acut.,' § 5, tome ii, p. 262, l. 7, ed. Littré.

[5] Rufus Ephes., p. 37, l. 18.

[6] 'Morb. Acut.,' ii, 28, § 147, p. 139, ed. Amman.

[7] 'De Carn.,' § 16, tome viii, p. 604, l. 8, ed. Littré.

[8] See his 'Œcon. Hippocr.'

[9] 'De Loc. Affect.,' i, 1, tom. viii, p. 2, ll. 7, ult.

[10] 'Caus. Morb. Chron.,' i, 10, p. 89, l. 8, ed. Ermerins; p. 73, l. 3, ed. Adams. The correction had previously been pointed out by Wigan and Petit.

[11] 'Consp. Med.,' iv, 17, in Ermerins, 'Anecd. Gr.,' p. 163, l. 11.

[12] Celsus, vii, 13, p. 289, ed. Daremberg; 'Galen, 'Defin. Med.,' c. 398, tom. xix, p. 443; Paulus Ægin., vi, 38, p. 184, ed. Briau; Aëtius, vi, 3, 6, p. 743, in H. Steph., 'Med. Artis Princ.'

[13] Dioscorides, 'Mat. Med.,' iv, 118, tom. 1, p. 606, l. 12, ed. Sprengel.

[14] See note to Theophilus, 'De Corp. Hum. Fabr.,' iii, 13, p. 106, l. 14, ed. Oxon.; and Index to Kühn's Galen.

[15] Aretæus, 'Caus. Acut.,' i, 6, p. 8, l. 26, ed. Adams. See Foës, 'Œcon. Hippocr.'

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THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

APRIL, 1869.

PART FIRST.

Analytical and Critical Reviews.

REVIEW I.

Clinical Lectures on Diseases of the Liver, Jaundice, and Abdominal Dropsy. By CHARLES MURCHISON, M.D., &c. London, 1868. Pp. 556.

AFTER the very elaborate treatise on diseases of the liver by Frerichs, which has been placed within easy reach of all English readers by the Sydenham Society's excellent translation, it seemed a bold venture for anyone to upturn anew the same ground for some years to come. Dr. Murchison has, however, attempted the task, and has brought out a book which, although, as its modest title imports, it does not vie with that of the great Berlin clinician in extent of research and literary completeness, surpasses, nevertheless, in clinical usefulness every work on diseases of the liver yet published. By a plan of arrangement quite his own the author has so packed his facts as to make them easy for all to remember, and to produce at will when wanted at the bed-side. Avoidance of all unnecessary detail, and a wholesome adherence to facts, particularly commend these lectures to the notice of every medical student and practitioner. In the still obscure question of the pathology of jaundice he is, it is true, compelled to lean on what he regards as the most probable of hypotheses: yet his rule is to make no statement which he cannot directly back by reference to cases in his wards, or to the post-mortem table. Hence a plan, adopted of late in several of our best medical works, and already carried out by him, though on a smaller scale, in his great book on fevers, has been followed on a large scale in the present lectures;—that, namely of illustrating each subject by picked and well-recorded cases, either from the wards or from private practice. This method of instruction has for the student the double value of illustration and of guidance in case-taking. The book before us contains no less than ninety-six cases, the greater number of which are related at length, and some of which are pattern clinical records, seizing on important symptoms, noticing where necessary absent symptoms, and zealously avoiding redundance or repetition. Many of the cases are still further perfected by the aid of diagrammatic figures[1] for marking out physical diagnosis.

The Lectures, twelve in number, delivered at the Middlesex Hospital, have for their main object "to determine the diagnostic import of those signs and symptoms—such as enlargement of the liver, jaundice, dropsy, and pain—which are common to many different hepatic disorders, but the precise cause of which is too often unrecognised." In Lecture I, the normal dimensions of the liver and the spurious or counterfeit enlargements of the organ are considered. Careful rules are laid down to enable the student to distinguish the latter from true enlargements, which are fully discussed in Lecture II. Dr. Murchison has here hit upon the happiest arrangement yet made, by dividing enlargements of the liver into *painless* and *painful*. In the former category are grouped the waxy and fatty liver, hydatids, and hypertrophy. In the latter, congestion, catarrh of the ducts, obstruction of the common bile-duct and retention of bile, abscess, and cancer are included. In treating of the waxy liver, he makes the same appeal to the surgeon that is made by Dr. Dickinson on behalf of the kidneys, in his excellent monograph[2]—to endeavour, namely, to arrest or shorten as far as possible suppuration from any part of the body, especially from bone. Both these physicians allude more particularly to resection of joints as tending to bring on this waxy degeneration in internal organs, and question how far, from their point of view, this operation can be called conservative.

Lecture III, one of the ablest in the book, is devoted to the *hydatid tumour*. The rule is laid down, and may now be regarded as established, that whenever the tumour "is large enough to be recognised during life and is increasing in size," it is to be tapped with a *fine* trocar, and the fluid contents of the cyst are to be withdrawn; by which means both the parent hydatid and its offspring are destroyed. A spot is to be selected where the fluid comes nearest to the surface, and the trocar is here introduced. The cannula is to be removed before the whole of the fluid is drawn off, in order to prevent the admission of air; and, in the removal, the very necessary precaution of pressing the punctured portion of the abdominal wall against the cyst must be taken; otherwise, "the abdominal wall will be pulled away from the cyst in the extraction of the cannula, and the fluid in the cannula may drop into the peritoneum." Dr. Budd's plan of suction by means of an exhausting syringe adapted to the cannula has not answered in Dr. Murchison's hands, on account of the pain which the proceeding seems to cause. We would remark here that Dr. Roberts, of Manchester, is in the habit of adapting a long elastic tube to the cannula after withdrawing the trocar, and so avoiding admission of air. He employs trocars of the smallest possible size. He exhibited at the last meeting of the British Medical Association at Oxford a very ingenious and compact case of instruments for this kind of tapping, and made a most interesting communication on the subject of "exploration and tapping." Dr. Murchison's analysis of cases shows the above-mentioned operation to have been perfectly successful in thirty-five of forty-six cases,—a result which strongly recommends it to all. The characters by which the fluid of a hydatid cyst may, after removal by an exploratory puncture, be with certainty recognised, are as follows:

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[1] Outline figures of this kind, most valuable to all who keep clinical notes, may be bought, in neat little books of 12, 24, and 36 sets (price 1*s*., 1*s*. 6*d*., and 2*s*.), or in loose sheets, at sixpence per dozen sets, of Maclachlan and Stewart, Edinburgh.

[2] 'Pathology and Treatment of Albuminuria,' p. 212.

> "If the sac be not inflamed, the fluid is limpid when running in a stream, with a slight opalescence when viewed in bulk; it is alkaline, and has a specific gravity of 1007 or 1009; it contains neither albumen nor urea, but throws down a copious white precipitate with nitrate of silver, owing to its strong impregnation with common salt."

Among the painful enlargements of the liver there is nothing which specially attracts notice in the remarks on congestion of the liver, inflammation of the bile-ducts, and obstruction of the bile-ducts.

The subject of *abscess*, in Lecture V, deserves attention; for the author, familiar as he is with the diseases of India, speaks here with undeniable authority. He distinguishes the *pyæmic* from the *tropical* abscess. The latter is certainly in many instances pyæmic—following on dysentery,—as Budd has shown; but, on the other hand, it occurs so often independently of any such cause, and simply as a consequence of inflammation (as Murchison stated, fourteen years ago, in his 'Observations on the Climate and Diseases of Burmah'), that it may be set down as a distinct form of disease. It is, as a rule, single, large, and, contrasted with the European abscess, rarely attended with jaundice. The pyæmic, or European, abscess is, on the contrary, multiple, small, as a rule, and, in the majority of cases, attended with jaundice. The advantages and disadvantages of operative interference in the tropical abscess are fully and temperately discussed, and the conclusion is arrived at that an operation is advisable in a large number of cases.

a. "In all cases where there is a visible fluctuating tumour, operate at once.

b. "In cases where the symptoms of abscess are present, with a distinct tumour projecting from the normal contour of the liver, or causing bulging of the ribs, even though there be no perceptible fluctuation, it will be well to operate.

c. "When symptoms of abscess coexist with uniform enlargement of the liver, but with no distinct tumour or bulging, if there be any local œdema, or obliteration of an intercostal space, or acute pain, always localized to one particular spot when the patient takes a full inspiration, it will be well to operate; but if there be no such œdema or obliteration or pain, it may be better to wait, as the enlargement may possibly be due to multiple abscesses, or, if there be but one abscess, it is doubtful if it will be reached."

Passing from the enlargements, we are brought, in Lecture VII, to the consideration of the different forms of *contraction* of the liver. Of these there are three principal,—the simple contraction (of old age or inanition); the acute, or yellow, atrophy; the chronic atrophy. He agrees with Förster in believing that there are two forms of granular liver or cirrhosis, one in which there is a great density of structure from a superabundance of connective tissue; another in which, although there is a like granular or nodular appearance of the surface, there is no increase of connective tissue, and the texture of the organ is even softer than in health. It is, then, the consistence which distinguishes the one from the other. The former is the "gin-liver;" the etiology of the latter is obscure, but alcohol is not the cause.

The introductory remarks on *jaundice* in Lecture VIII are very sensible, showing, as they do, how medical men are too apt to be satisfied with the fact that a patient has got jaundice, and to plunge at once into the administration of cholagogues, as they are termed, without reasoning on the probable cause of the yellow colour of the patient and reflecting how far the mischief may be aggravated by active treatment. Purgatives, and mercury among their number, do give relief in many forms of jaundice, but we have no evidence to show that they do so by any "cholagogue" action that they possess. Were mercury a cholagogue, which there is every reason to suppose it is not, it would in many cases of jaundice aggravate the mischief a thousandfold by adding to the quantity of bile which is already dammed up by some obstruction to its exit, and is injuring by distension both the bile-ducts and gall-bladder. An unnatural slowness of the pulse is a symptom noticed in some, but not in all, cases of jaundice. Röhrig's ingenious experiments on rabbits and dogs show that if a solution of cholate or glycocholate of soda be injected into the blood of these animals, the pulse at once begins to flag, but that glycine, taurine, cholepyrrhine, bilifulvine, and cholestearine have no power to produce this effect. He concludes, then, that certain constituents of the bile—biliary acid salts—act as poisons on the nerve ganglia of the heart. Traube[1] has arrived at the same results, but offers another explanation of the phenomenon. Finding, as he does, that, even after the division of the pneumogastric nerves, these salts still produce the same paralyzing effect on the heart, he concludes that it is on the muscular tissue of the heart, and not on the nerve ganglia, that the bile exerts an abnormal influence. It is probable, therefore, that in those cases of jaundice which are characterised by slowness of the pulse, the biliary acids, as well as the bile-pigment, are present in the blood.

Setting aside the larger number of instances in which jaundice is caused by some mechanical or other obstruction of the bile-duct—either by blocking up of its calibre, or by stricture, or by compression,— the pathology of which is clear to all, one turns eagerly, in any new work on the subject, to seek, if possible, some fresh, some clearer, interpretation of the jaundice which cannot be explained by any impediment to the escape of bile, where during life the fæces are bile-stained, and where post-mortem examination finds the ducts free from their origin to their termination in the duodenum. The common hypothesis of "jaundice from suppression" has never been satisfactory. It assumes that the elements of the bile, or at least the bile-pigment, pre-exist in the blood, and are merely excreted, not manufactured, by the liver; that the liver when influenced by certain pernicious agents, as blood-poisons and the like, ceases for a while this excretory action; and that, consequently, the escape of the bile-elements from the blood is temporarily checked or "suppressed." Hence an accumulation of bile pigment in the blood, and a staining of the tissues therewith. Against this hypothesis may be arrayed some very strong arguments:—1st. "That it has not yet been satisfactorily shown that bile-pigment exists ready formed in the blood of persons who have not jaundice;" 2nd. That extirpation of the entire liver in animals is not followed by jaundice; 3rd. That the liver-tissue may be almost entirely destroyed (in advanced cancer, cirrhosis, fatty and waxy degeneration) so that the gall-bladder contains nothing but a little mucus, and yet there may be no jaundice, and no bile in the urine. Two years ago, Dr. Bence Jones, in a very striking paper on the subject in question,[1] proposed a solution of this difficulty which carries great weight with it, not merely from the reputation of its author, but from its own intrinsic worth. He asserts that in health a certain quantity of bile is always being absorbed by means of a constant diffusion or transudation of bile into the blood through the coats of the blood-vessels in the gall-bladder. This absorbed bile is at once oxydized in healthy blood, and produces no ill effects. If, however, the normal changes in the blood be deranged by disease (blood-poisons, &c.), or more bile be absorbed than can be oxydized, bile accumulates in the blood and stains the tissues. On this supposition, then, before jaundice in any form can occur, bile must be manufactured by the liver; and all jaundice resolves itself into jaundice from suboxidation and jaundice from an excessive absorption of bile into the blood. Of "suppression of bile" we know nothing, for we are ignorant of the parent substances from which the bile is derived. This view of the matter is adopted by Murchison, who adduces evidence to show that, not only from the gall-bladder and bile-ducts, but also from the intestine, a continual osmosis of bile into the blood is going on in health. Dr. Harley's modification of Pettenkofer's test[2] for indicating the presence or absence of biliary acids is objected to as untrustworthy by Murchison. The latter applied it to the urine of six of his patients who were in the hospital together, and found that in three of the six the characteristic dark purple colour was formed at the line of junction of the acid with the urine. In two of the three there was no jaundice, nor any disease of the liver; in the third there was jaundice from obstruction. The colour in the latter, he says, could not possibly be distinguished from that in the two former.

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[1] 'Berlin Klin. Wochenschr.,' 1864, 9 und 15.

Two different conditions, under which jaundice without obstruction may occur, require separate notice. The first of these is the jaundice of blood-poisoning. This is very much simplified by Virchow. He believes that there are always present, in pyæmia, typhus, pneumonia, phosphorus-poisoning, and other similar conditions, catarrhal changes in the stomach and duodenum, which will sometimes, when unusually severe, cause obstruction of the duct and jaundice, by extending to the lining membrane of the common duct itself. The mucous membrane around the orifice of the duct is swollen up from inflammation, and the intestinal portion of the duct is filled with a white, soft, but co-hærent, plug, which may be squeezed out of the orifice. It is possible that this explanation may serve for some of these cases, but it will not suffice for all. Dr. Murchison's reported cases give convincing proof that jaundice may occur in connection with pyæmia, typhus, typhoid, and scarlet fever, and yet no trace of obstruction be found in the bile-duct on a searching post-mortem examination. Such instances as these—as well as the numerous examples of yellow and remittent fever which have been recorded by other observers, and in which there has been an abundant discharge of bile in the fæces as well as an entire absence of any obstruction after death—must be regarded as cases of jaundice from suboxidation. In the same category Murchison would place the *acute yellow atrophy* of the liver: for he is inclined to look on the atrophy in this disease as one among many other signs of some great blood-disturbance, rather than as the cause of the terrible concomitant symptoms. He points out the very close resemblance that the disease bears, both clinically and pathologically, to phosphorus-poisoning; so much so as to raise a suspicion (Wagner) "that many of the recorded instances of acute atrophy were probably cases of acute poisoning by phosphorus." He adverts to the fact that leucine and tyrosine are found in the jaundice of typhus as in that of acute atrophy, and that, in a case of enteric fever complicated with jaundice, the liver has been found in a state of acute atrophy. He shows that several residents in a house have been attacked almost simultaneously with acute atrophy, and that this disease has at other times shown itself in an epidemic form. Lastly, the granular degeneration of the heart and renal epithelium in typhus, the general fatty degeneration of all tissues in phosphorus-poisoning, and the fatty change in the heart and kidneys in acute atrophy, all combine to strengthen the suspicion that this mysterious and fearful disease is caused by the presence of some poison in the blood. The second variety of jaundice to which we would call attention, is that *which immediately follows some fright or other mental emotion.* The author here believes that the onset of the jaundice is too rapid to be accounted for by any obstruction from spasm of the ducts or congestion of the liver. He believes, rather, that the natural metamorphoses in the blood are deranged or checked suddenly by nervous influence, so that this form of jaundice would be referred to the same group as the preceding. Dr. Bence Jones supposes that the effect of the shock or nervous irritation is to cause an excessive formation of bile, that the gall-bladder becomes unduly distended, and that more bile passes into the blood than can be oxydized.

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[1] 'St. George's Hospital Reports, 1866,' p. 189.

[2] "To a couple of drachms of the suspected urine add a small fragment of loaf sugar, and afterwards pour slowly into the test-tube about a drachm of strong sulphuric acid. This should be done so as not to mix the two liquids. If biliary acids be present, there will be observed at the line of contact of the acid and urine—after standing for a few minutes—a deep purple hue. This result may be taken as a sure indication that the jaundice is due to obstructed bile-ducts." On the other hand, "a brown instead of a purple tint" is said to be equally indicative of suppression.

By thus classifying cases of jaundice in acute yellow atrophy of the liver with those of jaundice in fevers, in pyæmia, in phosphorus-poisoning, &c., and regarding them all alike as instances of jaundice from arrested metamorphosis of bile, we greatly facilitate the explanation of those serious cerebral symptoms which are apt to supervene in connection with all these forms of jaundice. The same poison in the blood which prevents the bile from being oxydized, prevents the urea from being properly elaborated; and hence the accumulation in the blood of substances which should be excreted—as evidenced by the presence of leucine and tyrosine—and by which the functions of the nervous system are entirely deranged. How far the changes in the liver itself can be directly regarded as playing an important, or indeed any, part in the causation of these brain symptoms is a question. Dr. Murchison is of opinion that the liver does play such a part; that "the arrest of the functions of the liver checks or modifies the metamorphosis" which ought to be going on in the blood, and adds to the difficulties already present in the way of any proper formation of urea. In this he may be right: but he does not, we would with all deference submit, lay enough stress on another prominent agent in the production of the cerebral disturbance, namely on the concomitant renal disease. We have already alluded to the co-existence of renal disease with acute atrophy of the liver, to the granular degeneration of the renal epithelium in typhus, and to the fatty changes of the kidney in poisoning from phosphorus. In the latter, indeed, the disease of the kidney is quite as marked a post-mortem change as that of the liver. Virchow, again, speaks very strongly on this question. We find in some notes of clinical lectures that we heard him deliver some few years ago, the following statement:—that no condition of the liver itself will account for these serious cerebral symptoms, but that there are always, simultaneously present, signs of disease in the kidneys. He has, he said, never failed to find well-marked changes in the kidney in all the cases of "icterus gravis" that have come under his observation. To sum up, then, we believe that the cerebral symptoms which supervene in cases of severe jaundice are due to the presence of substances in the blood which should be excreted; and that this imperfect excretion depends partly on the presence of a blood-poison which interferes with healthy oxidation and elaboration of urea, partly on disease of the kidneys co-existing with disease of the liver and other organs of the body. Whether the atrophy of the liver has of itself anything to do with these symptoms is a doubtful matter.

This valuable chapter on jaundice closes with some useful memoranda for diagnosis, conveniently arranged. They are too full to quote, but are so well put as to be strongly recommended to all practitioners.

The two last lectures deal, the one with "Fluid in the Peritoneum," the conditions which simulate it, its causes and its treatment; the other with "Hepatic Pain," "Gall-stones," and "Enlargements of the Gall-bladder." Both lectures are full of information, and arranged with much method; but the space allotted to us forbids any further notice of them. The dangerous consequences to which gall-stones may lead are dwelt upon with much earnestness, and a warning is given against regarding them as harmless or forming too favourable a prognosis in a case where they are believed to be present.

REVIEW II.

1. *Statistical Report of the Health of the Navy for* 1865. 8vo, pp. 446.
2. *Statistical Report of the Health of the Navy for* 1866. 8vo, pp. 565.

1865 and 1866 were memorable epidemiological years. Apart from other considerations—such as the appearance, after an interval of nearly a century, of the cattle plague in this country, the partial spreading of yellow fever from an infected ship at a port in Wales, the epidemic prevalence of cerebro-myelitis in several districts in Germany, and its limited or sporadic manifestation in England and Ireland—the development and growth of the fourth great migratory movement of malignant cholera over vast regions of the Asiatic continent and ocean, and of Europe and America, suffice to stamp them with more than ordinary interest. For the first time in the history of this remarkable pestilence, has the profession been furnished with a large amount of authentic contemporaneous information relating to its course and career over the face of the globe; thanks to the measures adopted by the medical department of the Privy Council for utilising the despatches of British consuls, &c., in different parts of the world. Much, indeed, remains to be known for the elucidation of the many obscure questions which still surround its history; but a good example in the right direction has now been set, and this cannot fail of eventually leading to important results, if medical men everywhere would but become more accurate observers and more faithful chroniclers of facts, independently of all reference to any speculation or theory.

The annals of the health of our navy are sure to contribute some valuable memoranda to the general stock of information respecting every widespread epidemic. In 1865, there appears to have been a marked increase of bowel disorders on very many of the stations. On the home station diarrhœa was more prevalent than usual, and eight cases of cholera, mostly severe choleraic diarrhœa, were recorded. Only one case proved fatal, and that was of a doubtful character. In a well-marked case which occurred at Portsmouth in a seaman who, when drunk, "had lain down to sleep in the privy of a brothel in one of the lowest haunts of vice," the symptoms gave way under treatment on board the "Victory."

In the Mediterranean squadron the number of cases of diarrhœa was considerably in excess, and there were eleven cases of cholera, of which seven proved fatal. Four cases occurred in the "Assurance," at the mouth of the Danube, where the disease was prevalent at the time. Two occurred in the "Chanticleer," in Malta harbour, in July; and two in the "Cossack," at sea, in June, after leaving Alexandria. A single case occurred in the "Cockatrice," at Galatz, in June; and one among the crew of the "Hibernia," on shore at Malta. In the Naval Hospital there, a phthisical patient admitted in August died from an attack of cholera three weeks after, "though communication from without was very limited and indirect." The reports of the surgeon and assistant of the "Hibernia" are valuable contributions to the history of the epidemic in that island. Although moored within a hundred yards on each side of districts ravaged by the pestilence, and although leave was freely given to the crew to go on shore, the ship remained throughout the whole period free from any fatal attack. This immunity was, doubtless, largely due to the precaution, together with other sanitary appliances, of promptly arresting the slightest sickness; "for, in many instances on shore, a slight diarrhœa neglected led the way to fatal results, and the least complaint made by any of our men was immediately attended to."

On the West India station, bowel disorders were above the average of ordinary years. Diarrhœa in the "Fawn," at Nassau, during hot sultry weather in May, was "characterised by a strong tendency to collapse in the more severe cases. It was profuse, generally painless, but easily checked." Diarrhœa was prevalent at Bermuda in May and June; also in the "Duncan," at Halifax, in the autumn. One case of choleraic diarrhœa was fatal on board. On the Brazilian, and likewise on the Pacific, station, allusion is made to the prevalence of diarrhœa at

Rio Janeiro, Monte Video, and Valparaiso. In the East Indian squadron, the number of cases of and deaths from cholera was considerably greater than in the previous year. In April and May attacks occurred in the "Penguin," at sea, between Aden and the opposite African coast; also at Bombay, "where the disease was prevailing extensively, both on shore and afloat." At Calcutta it was prevalent on shore when the "Highflyer" contracted the disease. In the China squadron, too, the number of cases and deaths from this cause was twice as great as in 1864. Cases occurred at Hongkong, Amoy, Shanghai, Manilla, Borneo, and Penang.

The above memoranda, although insufficient certainly to indicate by themselves the existence of a pandemic 'medical constitution' in 1865, are, nevertheless, not unworthy of notice as items of information respecting a remarkable epidemic year.

The experience of the navy of recent years, in respect of cholera, has certainly been very satisfactory, and reflects no small credit on the hygienic arrangements made on board of our ships of war to counteract and check the power of its malignancy.

"It is a disease," says Dr. Bryson, "which is entirely under our control, as it may be arrested in its progress by ventilation and segregation, with such other quarantine restrictions as will dissipate or lessen the force of the poison."

But there is another pestilence which continues to inflict grievous disasters upon our seamen, and in the prevention and arrest of which on board ship no progress, unhappily, has been made of late years. On the contrary, the amount of fatal sickness from yellow fever in our navy, in the West Indian and West African stations, during the last two decades, has been decidedly greater than it was previously. This statement may surprise many of our readers, as we are continually being assured in Parliament, and through other public channels, that the sickness and death-rates in the navy are wonderfully low, and that the general health-state of the service altogether is most satisfactory. The above fact is, however, indisputable; and it behoves those who are best acquainted with the details respecting its recent history in ships of war to endeavour to discover the cause of the evil, and to suggest a remedy for so serious a calamity. Unfortunately, there is still as much controversy as ever among medical men, official and non-official, as to the nature and attributes of this disease, and, consequently, as to the proper measures for arresting its development and spread.

The views of the Admiralty authorities are, it is well known, not in accord with those of many of the most experienced medical officers of the service; and, what is more, the official opinions now dominant are at variance with what were in force not many years ago, at a time when a sharp feud existed on this very subject between two departments of the Government. Whence this conflict and fluctuation of doctrine about phenomena in a branch of physical research, it may be reasonably asked, if inquirers confined themselves to strict and repeated observation, and to cautious and mature induction, unbiassed by theory or prejudice? It is now long since the subject was discussed at any length in this Journal, and the scope of our remarks on that occasion was to expose the fallacy of official one-sidedness. Since then, so much fresh material has accumulated, and especially by the publication of the 'Annual Reports of the Navy,' beginning with 1856, that we cannot but believe that a re-examination of so important a topic will be acceptable to the profession. In doing this, we shall first adduce some highly interesting evidence, circumstantial and unquestionable, from the history of certain remarkable outbreaks in our ships of war, and then draw from it such simple conclusions as obviously suggest themselves.

The first case is taken from the Report for 1865.

The steamship "Archer," with a crew of 190, had been on the West African station since July, 1863. In 1864 her sick-rate was higher than that of any other ship of the squadron, from the prevalence of fever, catarrh, bowel-complaints, and abscesses. This general sickness continued during the first nine months of 1865. Diarrhœa was prevalent in the Michaelmas quarter. In September she was at Loando, in latitude four degrees south. A small party of the crew lived there on shore, for the purpose of repairing the engines. With this party was associated a man, Lynch by name, who had been left by the "Ranger" in charge of a prize vessel which had been captured. Lynch and two of the other men contracted fever at Loando, and were taken on board the "Pioneer," a small local steamer, tender to the "Archer." In consequence of the surgeon of the latter being ill with fever, and there being no other medical officer of the ship, the assistant-surgeon in charge of the tender came on board, bringing with him the sick men. Lynch was at that time, October 6th, "in a low typhoid state, attended by slight stupor, nervous depression, and great irritability of the stomach." He became worse and worse, and died on the 8th, with coma and black vomit, in the steerage, "in a hammock opposite the midshipmen's berth." After going to the mouth of the River Congo, the "Archer" returned to Loando, and embarked the rest of the working party still on shore; and on the 25th she sailed for the island of Ascension, which was reached on November 4th. By this time twenty-nine fresh cases of fever of various types had occurred, and five had been fatal with black vomit, &c., viz., one on the 26th, the 28th, the 30th, and on November 2nd and 3rd. None of the fatal cases seem to have been among the men of the working party on shore.

Meanwhile, a fatal case had also occurred in the "Pioneer," in a young man who sickened on the 14th of October (the day after he left the "Archer" at the Congo), and who died on the 18th.[1]

On the arrival of the latter at Ascension, there were seventeen fever patients, and as many with other maladies, on her sick list. The sick who could be moved, and all the convalescents, were at once landed; those who were too ill "were removed into the bows, where they had the full benefit of the fresh breeze." Five only of the crew, along with some Kroomen, were left on board to purify and disinfect the ship; "but, unfortunately, a party from the shore was sent on board daily to assist in cleaning out the holds, &c."[2] Among these men fresh attacks of fever took place almost every day. Up to December 9th, when she sailed for England, about forty new cases in all occurred; and of these eight were fatal. The disease did not spread to any of the small garrison or other residents on the island, suitable precautions having been taken to prevent communication with the ship. On the voyage home—she reached Plymouth on January 9th—there were twelve fresh cases, and four deaths. The last death occurred on December 24th. "Between October 6th and December 20th, when the last attack occurred, ninety-seven cases of fever were placed on the sick list. Of these, twenty-eight were returned as remittent, fifty-six as yellow fever, and thirteen as ephemera. There were nineteen deaths altogether." Among the victims were the assistant-surgeon, and five out of seven midshipmen. "The disease appeared to be especially confined to the steerage, as all the men who kept sentry in that part of the ship said they were taken ill whilst on duty." The "Archer" was, soon after her return, condemned to be broken up, in consequence of the unsound state of the timbers of her hull, although she had not been above ten years in the service.

As to the probable origin of this outbreak of yellow fever, our information is far from being so complete or so authentic as could be wished or might have been expected. The surgeon does not even state whether the disease was known to exist at

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[1] A death from what is termed "malignant remittent fever" had previously occurred, viz., during the Midsummer quarter, in a man who had been employed at work in the "Pioneer."

[2] Nothing is stated as to the sanitary condition of the ship. The purification must, of course, have been very imperfect, as part of the crew was on board all the time.

Loando when the ship was there. All that we learn on this most important point is in the 'Navy Abstract,' published a year before the present report, that the fever "had been previously at Loando, and some deaths had occurred in a Portuguese man-of-war there." But no evidence or authority whatever for this statement is cited; and the other ships of the British squadron ("Pandora," "Ranger," and "Rattlesnake") which had visited Loando seem to have altogether escaped. Neither did the working party of the "Archer's" crew on shore suffer, it would appear, from the fatal form of fever.

On the same station, and in the same year, there occurred a severe outbreak in a ship just arrived from England, the record of which will ever form one of the most interesting and instructive pages in the history of yellow fever. All the details connected with this remarkable visitation deserve to be followed out from beginning to end. It will be necessary first to make the reader acquainted with what took place in another ship.

The "Isis," an old frigate hulk, had been the receiving ship since 1861, stationed at Sierra Leone; she was moored about 200 yards from the shore. Her ordinary crew did not exceed twenty-eight, and of these not more than six were usually Europeans. The Michaelmas quarter of 1865 had been very sickly on shore—more than a fourth of the European population died of "fever of a very malignant type." There were numerous cases of fever in the "Isis" at the same time; these were registered as remittent. One, however, proved fatal, in the person of the ship's steward, who died on September 28th with black vomit. The next fatal case on board was on October 20th, in a young man recently from England, and who had joined the "Isis" only on the 14th of that month, and sickened on the 18th. Another young man who had arrived at the same time as this patient, and who had joined the ship on the 12th, was placed on the sick list on the 19th; and, after a deceptive amendment, died on the 27th, delirious and with hæmorrhage from the mouth. These two patients had clearly contracted the fever on board the "Isis." Next month, November, the "Rattlesnake" arrived from Ascension at Sierra Leone on the 22nd; and, in consequence of the serious illness of her surgeon, she had to engage the services of the assistant-surgeon of the "Isis." One of the men of the "Rattlesnake," doing duty in the "Isis," was attacked with yellow fever on the 27th, and died four days afterwards. While the "Rattlesnake" remained at Sierra Leone till December 24th, when she sailed for England, five deaths from yellow fever occurred on board, viz. on the 16th, 20th, 24th, 25th, and 26th of December, all of them in persons who were known, or believed, to have been on board the

"Isis." Besides these five fatal cases, there were seventeen cases returned as remittent or ephemeral fevers among the crew: all these recovered. The sickness in the "Rattlesnake" promptly ceased after she left the coast.

Prior to that date, two more fatal cases had occurred in the "Isis" in persons fresh from England. A naval commander and his servant arrived, by the mail steamer, on December 12th to join the "Sparrow" on the station. They both went on board the "Isis" on the 13th. The servant sickened on the 15th, and was dead on the 21st. His master sickened on the 18th, and died also on the 21st. Such was the deadly power of the fever poison in this floating pest-house. But our tale is not at an end. On the very day after the sailing of the "Rattlesnake," the "Bristol," with a crew of 500, arrived fresh from England at Sierra Leone. Freetown was then considered to be comparatively healthy. Three days after her arrival, 112 of her crew with four officers were employed in removing the "Isis" from her mooring to a more healthy part of the harbour; "it being considered not improbable that her proximity to the shore had something to do with the persistence of the fatal attacks" of yellow fever.[1] They were thus engaged on the 28th and 29th December. With the exception of some of the officers, and a small party who landed on the 29th to purchase provisions, no communication was held with the shore. On the 30th the "Bristol" left for Monrovia. On that day an officer, who had been ashore on the 26th and had passed the night on board the "Isis," was put on the sick list (he had been taken ill on the 29th), and had a most severe attack, but recovered. Cases rapidly followed; as many as twenty seizures occurred on January 1st, and fifteen more between that date and the 6th. There was then a lull for six days; the last attack was on the 12th. The total number of cases was "thirty-eight, of which twenty-nine have been classed as cases of yellow fever, and nine of remittent." Twenty-one deaths had occurred on board by January 10th; the last two deaths took place on shore at Ascension (whither the ship's course had been directed upon the outbreak of sickness), which was reached on the 13th. The whole of the persons attacked had belonged to the working party sent on board the "Isis," with the exception of one man; and he was one of a boat's crew who, on returning from the shore, had gone on board the "Isis." The rest of the crew, who did not leave the boat, escaped. On reaching Ascension, the sick were at once landed; and no further extension of the disease occurred. During the voyage the sick were kept separated from the ship's company by canvas screens; the whole of the fore part of the ship, as far aft as the galley, and the space between the two ports abaft the sick bay, being entirely set apart for fever cases. These precautions for isolating the sick were, as the surgeon remarks, "the best that could be adopted under the circumstances; still they were inadequate to have prevented the spread of a contagious disease." He adds:

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[1] It would almost seem, from the following remarks, that this step had been taken by the Admiralty without the concurrence or approval of the Medical Department. "The uncertainty which still exists in the minds of some as to the communicability of yellow fever renders it little matter of surprise that it should be determined to move the "Isis" to what was supposed to be a more healthy locality; but there can be little question that the existence of the fever poison in that vessel depended neither on the locality nor on the water used in the ship, to which some reference was made by the medical officers, but on a specific poison previously emanating from the bodies of yellow-fever patients on board."

> "The men employed as nurses, numbering some twenty-four, who were hanging for hours over, and supporting in their arms, their dying shipmates, their frocks in many cases accidentally covered with black vomit, none of these men suffered from fever; nor did any of the five medical officers, who were in constant attendance on the sick. Also, on the removal of the sick to hospital at Ascension, not a single case occurred either among the nurses or the medical officers of that establishment."

Thus, within six or seven weeks, no fewer than twenty-eight deaths among the crews of two ships of war from this malignant fever were clearly due to communication with the "Isis;" all these deaths occurring exclusively among men who had gone on board that vessel. It was certainly high time that "positive instructions were now issued that no fresh arrivals from England, nor supernumeraries of any kind, were to be permitted to go on board the 'Isis,' and all communication between that vessel and the squadron ceased." It is to be hoped that not only a complete report of the medical history of this receiving vessel since she has been at Sierra Leone may have been ordered by the Admiralty, but that, for the sake of our seamen, proper measures have been adopted for her thorough overhauling and purification, if she is to continue in the service.[1] Had such a disaster occurred in the army, there cannot be a doubt but that a searching inquiry would have been ordered forthwith.

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[1] The condition of the receiving-ship at Rio Janeiro, the "Madagascar," which had long been infested with yellow fever, was found on inspection to be as follows:—"The sides of the ship and the lining were in many places decayed, damp, and rotten; and, on lifting the limber boards, a quantity of black offensive mud was discovered, the smell of which caused nausea, vomiting, and diarrhœa, in several of the officers present." The surgeon expressed his conviction that this state of things "did much mischief, predisposing men to contract the fever, infection being present."—'Rep. for 1859,' p. 68.

The incubation of the disease could not have exceeded from twenty-four to thirty-six hours in some of the cases contracted on board the "Isis." Still shorter was the period believed to be in a case of a boy who went on board the next ship, whose history we proceed to give, and to which frequent reference is made in the 'Navy Reports.'

The steamship "Icarus," with a crew of 110, arrived from England in the West Indies at the end of 1859. Throughout 1860 she was extremely sickly. She had a larger number of cases of fever (independently of yellow fever) than any other ship on the station; and her general sick-rate from other maladies was also very high. The assistant-surgeon died on board, at sea, of yellow fever, on June 27th.[1] He sickened on the 24th, four days after leaving Port Royal. Then and subsequently, throughout July, a large number of the crew were ill with fever of various types, periodic and continued. In August, the ship was at Belize from the 1st to the 15th. Yellow fever is stated to have "just begun to show itself there." She afterwards went to Omoa and Truxillo, on the coast of Honduras; these places were reported healthy. At the latter place a man was attacked on the 23rd with fever, which proved fatal on the 27th. It was not, however, for nearly three weeks more that the "fatal epidemic of yellow fever broke out in the 'Icarus,' commencing on September 16th," and which rapidly prevailed, and with so great malignancy that, when she reached Port Royal on October 9th, she had already lost thirty-two of her crew, and there were forty-nine sick or convalescent on board. With such an amount of sickness and loss of life on the voyage from Belize, it is no wonder that it was only with difficulty that the ship reached Jamaica. The sick and convalescent were at once sent to the hospital on shore, while the healthy part of the crew were sent on board a hulk in the harbour. As the ship was reported to be in a filthy state, she was cleared out and purified by negroes hired for the purpose. Five deaths occurred among the men sent to hospital. Thus, out of a crew of 110, no fewer than thirty-nine had perished from yellow fever since the first case in June. Amongst the victims were two assistant-surgeons. The "Icarus" left Port Royal on October 28th for England, which was reached without any recurrence of fever on board. But the interest of her story does not end here. On the 12th, the third day after she reached

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[1] He had visited cases of yellow fever at Port-au-Prince on the 11th June. The "Icarus" was at Port Royal from the 16th to the 20th. A few cases of yellow fever seem to have occurred about this time in Port Royal and Kingston. A marine of the "Imaum" died in the latter part of June at Port Royal; and the death of a soldier there is mentioned in the Army Report.

Port Royal, fourteen lads, recently from England, were sent from the "Imaum" (receiving-ship) to unbend her sails. Being caught by a heavy rain, they had gone below for shelter. Of these youths seven were attacked on the 14th, and six more within the next four days. They were removed from the "Imaum" to the hospital, where most of them died. From them the fever spread to others on board the receiving-ship. Between the 20th and 26th October, nineteen cases were sent from her to hospital; and of these only one had been on board the "Icarus." Of thirty-nine cases received in the hospital from the "Imaum," seventeen were fatal; and of a total of ninety-four patients, sent from all the infected vessels, twenty-two died and two were invalided. At the last-mentioned date, when there were between seventy and eighty cases of yellow fever in the hospital, one of the ordinary patients, affected with ulcer, was attacked; and he died four days afterwards with black vomit. Three days later, a boy of the "Hydra," then lying at Port Royal, who had been sent to hospital with diarrhœa, sickened with the fever, and died on the seventh day of the attack. None of the medical officers and other attendants in the hospital suffered. A few scattered cases of fever had occurred on shore, both at Port Royal and in Kingston. There still remains another episode in connection with the "Icarus" outbreak, which must not be passed over, viz., the story of the fever in the "Barraconta," another of the West India squadron, then at Port Royal. And this is the more necessary, as the narrative of the events in this ship bears on the hygienic administration of the Navy in epidemic emergencies, and involves, too, the consideration of some important principles of State Medicine in relation to quarantine.

The "Barraconta" arrived from Havanna at Port Royal on October 20th; her crew numbered 180. In consequence of the yellow fever then at Port Royal, 103 supernumeraries (men and boys) from the "Imaum" were, in order to get them away from the sphere of infection, sent on board on the 24th; and she sailed for Bermuda that afternoon. Next day, a boy of the "Barraconta" and one of the supernumeraries (who had been employed a whole day at work in the "Icarus") sickened; on the 26th, there were two fresh cases; two on the 27th; four on the 29th, and four on the 30th. A slight case occurred on Nov. 3rd, and another on the 4th. "By this time the disease had assumed a much milder type," when she reached Bermuda. In all, there had been sixteen cases, seven among the supernumeraries, and nine among the crew of the ship. Three of the cases had proved fatal. And now comes the incident which makes this case especially noteworthy. The commanding officer at Bermuda refused to allow the sick to be landed in the Naval hospital, or any communication, except by signal, to be held with the shore; and he directed that, after being provided by the quarantine officer of the port with necessary supplies, the ship should proceed to Halifax. Two days after leaving Bermuda, she encountered a heavy gale of wind, during which the temporary hospital on deck was blown to pieces and the sick were deluged with rain. "I cannot use language sufficiently strong," says the surgeon, "to convey an idea of the misery that the poor creatures endured." The captain, finding it impossible to reach Halifax, decided to make for England, which was reached on the 25th, a gale blowing all the way across the Atlantic. The ship was kept in quarantine at Spithead till the 27th, when the sick were sent to Haslar Hospital. Six cases of yellow fever, of which three were fatal, occurred during the voyage from Bermuda. The question will naturally present itself to the reader as to the legality, not to say the humanity, of the governor of Bermuda in the course he adopted; and the more so, as there is reason to believe that, it was in opposition to the opinion of the chief naval medical officer of the station. Before quitting the case of the "Icarus," it should be added that, after the disastrous sickness in 1860, she was anything but a healthy ship. In 1861, while on the home station, her sick list was much above the average, in part from an excess of fever cases; and in 1862, in the Mediterranean, she had an enormous number of cases of fever, and her general sick-rate was then the highest in the squadron.

In order that a comparison may be made between the narratives which have been now given, and that of the famous case of the "Eclair," which will always be regarded as a typical one in the history of yellow fever, it will be useful briefly to recapitulate its leading events. That steamer reached the African coast from England at the end of 1844. After touching at several points in the Bight of Benin, she arrived at Sierra Leone in February, and thence proceeded to explore the Sherboro' and Seebar rivers. The boats' crews employed in this service were much exposed to the weather in swampy localities, and cases of fever began to occur among them in the early part of April. Several of these were fatal. During May and June, many fresh attacks took place not only among the men who had been exposed in the boats, but in others who had not been out of the ship. Several of these cases were more of the continued than of the remittent type. In one, which proved fatal in June on the fourth day of the attack, the stomach was found on dissection "distended with serous fluid mixed with small black flakes." In July, the ship was at Sierra Leone from the 4th to the 23rd. While there, part of the crew were engaged in clearing out the dirt and rubbish from the hold of a vessel (the "Albert," that had been in the unfortunate Niger expedition), which had been lying there for a couple of years. Leave was also given at the time to the crew to go on shore; and, as usual, great intemperance and irregularities prevailed on these occasions. Yellow fever did not then exist, at least, epidemically, in the town; but July is usually the beginning of the sickly season. The first case of wellmarked black vomit occurred a day or two after leaving Sierra Leone, in a patient who had sickened a day or so before the ship sailed. Other fatal cases soon followed; in some, there were distinct febrile remissions; in others, the fever was more decidedly continued. After touching at the Gambia, and remaining there for five days, fresh cases occurred from day to day. It was then decided to proceed at once to Boa Vista, one of the Cape de Verde islands, and there land her crew, in hope of checking the fever, and in order to have the vessel cleansed and purified. Leave was granted by the Portuguese governor on arrival there on 21st August. Instead of camping the men, sick and well, out on an open area, they were most improperly all crowded together within an old fort, which was filthy and ill-ventilated. The fever continued to spread, and became more virulent and rapidly fatal than ever. Of thirty-seven fresh cases which occurred during the ship's stay, twenty-five were fatal. The crew were re-embarked on Sept. 13, and she sailed for England, which was not reached till the 28th, the fever persisting unabated during the voyage. On reaching the Motherbank, the medical authorities of Haslar Hospital recommended that the sick should be immediately sent on shore, if they could be safely removed, as well as all the convalescents; but this was objected to by the superintendent of quarantine, by whose advice no communication whatever with the shore was permitted, and the ship was ordered round to the Medway, to be there dealt with according to the rules of rigorous quarantine. This step was strongly condemned by Sir W. Burnett. It was not till October 3rd that the crew, sick and well, were got out of her; and, during the five days since her arrival in England, several fresh cases, some fatal, took place on board. Not a single instance occurred of the disease being communicated to any of the attendants upon the sick, out of the ship. The total number of deaths from the fever had considerably exceeded a third of the entire crew.

To efface the remembrance of this terrible disaster, the Admiralty changed the name of the ship to that of "Rosamond." But no sooner was she again sent to sea than a bad form of fever broke out on board; and, ever afterwards, she continued to be a notedly unhealthy vessel down to 1854 or 1855, when she was finally condemned.

Had any reasonable doubt previously existed as to the contagious property of yellow fever among the crowded crew of a ship of war—although the instances cited by Sir G. Blane, and the case of the "Bann" described by Sir W. Burnett, ought to have dissipated all doubts on the subject—the history of the "Eclair" indubitably settled the question in the affirmative. But it was far from determining other controverted points in the ætiological, and hygienic, relations of the disease. Nay, it rather served to stir them up for discussion; and this discussion might have assuredly led to most beneficial results in respect both of scientific inquiry, and of practical State medicine, had it only been conducted fairly and dispassionately.

The cardinal point of dispute then was, as it is now, whether the fever which had been so destructive, and had manifested unmistakable contagious qualities, was primordially a separate and specific entity, quite distinct from the fever which had been prevailing among the crew prior to the arrival of the ship at Sierra Leone in July; or whether it was still essentially this same fever, but with a superinduced malignancy, attributable to the agency of a poisoned atmosphere in the between-decks of the foul ship, from the infection of human and other exhalations among the crowded inmates. If the first view was correct, one difficulty was to determine when, and where, the new fever was contracted. Sir W. Pym maintained that it had been brought on board at Sierra Leone, and that all the cases previously had been simply of the remittent or ordinary coast fever. The two forms of fever were, according to him, entirely and utterly distinct, however much they might resemble each other in some of their features occasionally. On the other hand, Sir W. Burnett asserted that there was no evidence whatever of the introduction of this new form of fever; and he held that all the evidence went to show that the fever, which had been prevailing on board in May and June, had become envenomed, so to speak, or more and more malignant in type (acquiring, too, a contagious property), as the sickly season advanced, the hygienic condition of the crew deteriorated, and the sanitary state of the ship, more especially of the sick-bay, was made worse and worse by the ever-increasing numbers of the sick. This was the view which he had taken in the case of the "Bann,"[1] twenty years before; and the whole history of the "Eclair" went, he thought, to establish its truth. Dr. McWilliam, too, adopted the same opinion; one of the conclusions of his elaborate report on the subject was, "that the fever in the "Eclair" was primarily the remittent of the African coast, which is not contagious, but that the disease acquired contagious qualities in virtue of a series of causes." And Dr. Bryson, after declaring that "it has been clearly established that it [the fever] became contagious in the 'Bann,' 'Eden,' 'Forester,' and 'Eclair,'" added the important practical remark that "in each of these vessels that condition existed under which diseases, not in their simple form contagious, acquire the property of propagating themselves, such as erysipelas, ophthalmia, and synochal fever."[1]

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[1] "Although the fever did not at first appear to be contagious, yet, from want of ventilation and a great number of sick being confined in a small space, it ultimately became infectious."

The point here mooted, viz. that of diseases, not primarily and ordinarily contagious, being apt to become so at times and under certain circumstances or conditions, is one of extreme interest, and of the highest practical importance. This opinion was almost universally held in respect of malarial fevers, when occurring under unfavorable sanitary conditions, as in crowded ships and ill-formed hospitals, by the most experienced physicians of the army and navy, as well as in civil life, in the last century and in the early part of the present one. The doctrine pervades the work of Sir John Pringle on the 'Diseases of the Army,' and the same thing may be said of the writings of Lind in respect of the sister service. Quotations to the same effect might easily be cited from numerous other authorities, British and Continental;[2] but we shall at present confine our reference to the writings of Sir Gilbert Blane, as his attention had been specially drawn to the examination of the nature and character of yellow fever, and his great medical experience, both at sea and on shore, and in civil as well as in nautical life, en-titles his opinion to the highest consideration. His 'Select Dissertations' abound with such passages as these:

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[1] Besides the diseases enumerated by Dr. Bryson, there are several others decidedly liable to *become* contagious or infective. Dysentery is notedly so. Numerous proofs might be cited from the Navy Reports. The disease has, in several instances, been imported into our ships of war by captured African slaves being taken on board, as in the "Lyra" in 1861, and in the "Wasp" in 1865. The case of the "Conqueror" also, on the China station in 1865, is a good illustration. Nowhere, certainly, can stronger evidence be found of the gradual development of an acquired contagiousness in disease than in crowded and ill-ventilated ships. The typhus fever, which was imported into Liverpool with such calamitous results in February, 1861, by the Egyptian frigate from Alexandria, appears to have been generated on board from the accumulated mass of human beings in misery and filth during the voyage; and the same volume ('Trans. of Epidem. Soc.,' vol. i), containing a narrative of this case, records the remarkable history of an epidemic pleuropneumonia, which was suspected by the medical officer to have become to a certain extent communicable, in one of our line-of-battle ships in the Mediterranean in 1860. Her crew were unquestionably overcrowded, and the ventilation of the lower deck was very defective.

[2] No one has more pithily or more emphatically expressed his conviction on this point than Stahl: "Quid notius quam febris tertiana?; quid certius quam quod illa legitima longe absit a contagiosa communicatione?; quid vero familiarius quam ut etiam epidemice, imo contagiose, grassari observetur?"

> "There is a large class of infectious diseases which are not kept alive for ever like the specific contagions, but arising from a casual concurrence of circumstances, and then dying away. To these belong typhus fevers, dysentery, hospital ulcers, and ophthalmia. It is impossible that any one versant in civil, military, and medical history, can be ignorant of these facts. . . . . . Another line of distinction might be taken from diseases which are not infectious in their first stages, but become so in their progress. The typhus fever, including the yellow fever, may be adduced as an example of this."

And in his 'Essay on Yellow Fever,' he emphatically alludes to "the transition of the non-contagious into the contagious form, in consequence of foul air and filth in hospitals and ships, or other crowded places, as happens commonly enough in the fevers of Europe."

These views of Blane are, we believe, held by the most experienced officers of the army, as well as of the navy, at the present time.

It is a point of great practical interest, in respect of severe outbreaks of yellow fever on board ship, that most of the ships which have been most scourged in late years were unmistakably unhealthy vessels, as evidenced by their large excess of general sickness, not only during the yellow fever years, but also in those which preceded or followed them. This was the case, it has been seen, with the "Archer," the "Icarus," and the "Eclair;" and the same thing holds true of other vessels which have sustained fatal outbreaks of the fever. Thus, in the "Firebrand," which, in 1861, lost nearly a third of her entire crew, the sick-list was enormously high from the prevalence of ordinary fevers, fluxes, and abscesses, at the same time; and in 1862, when she had been moved from the West Indies to the Mediterranean, her sick-rate was largely in excess from fevers, diarrhœa, and catarrh. A similar remark might be made in respect of the "Spiteful" and of the "Jason,"[1] both of which suffered severely from yellow fever in 1861 in the West Indies. So it was with the "Malacca," which lost a third of her crew in a few weeks in 1856 on the same station, and also with the "Trident," which sustained a still heavier mortality on the African station in 1859. It is thus made clear—for many other instances of the same sort might be cited—that, in considering the ætiology of the origin and prevalence of yellow fever on board particular ships, the state of the general health of the crew prior to and during the outbreak should not be lost sight of. Outbreaks of the disease on shore have been almost invariably preceded by general sickness, and especially by the prevalence of diarrhœa and fevers.

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[1] A letter in the 'Times,' Sept. 18, 1861, from Halifax, where the "Jason" then was, states that "she is a new vessel built of green wood; her bilges cannot be kept sweet; the officers have tried all means to do so without success. This is considered the principal cause of her being so unhealthy. The stench is abominable, particularly in the aft part of the ship, and in the officers' cabins." The "Jason" is not the only sickly ship in which such a nuisance has existed.

A fruitful source of embarrassment in seeking to draw a distinct nosological line between the yellow fever and the endemic remittent of the West Indies and West African coast is the acknowledged truth that "it is, in fact, frequently difficult, and sometimes impossible, to distinguish the mild forms of yellow fever from the common endemic of the country (Jamaica), so like are they to each other."[1] This sentiment is continually being repeated in the reports of the medical officers; and, indeed, it will be observed that in every instance, without exception, of severe outbreak of yellow fever in ships, the invasion was preceded, accompanied, and often followed (as the disease subsided), with numerous cases of remittent fever among the crew, and that it was impossible to determine under what category many of them (those, in short, which did not end fatally in the black vomit) should be classed, whether as yellow fever, remittent, or synochus.[2] It is always to be remembered that the milder cases of yellow fever often exhibit distinct remissions, as in common malarial fever. Whether Dr. Mackay is justified in saying that "yellow fever in its symptoms may be as mild as any ephemeral fever, and yet confer as perfect an immunity from a second attack as the most virulent seizure," seems very doubtful. It is, moreover, recognised by all writers on the bilious remittent fevers of the West Indies, the Southern States of America, and the West Coast of Africa, that fatal cases are occasionally accompanied with black vomit, as well as with lurid yellowness of the surface, and that death occurs then in from two or three to five days of the attack. The experience of our ships of war affords many examples of this fact. In the Report for 1863, during which year "there was not an undoubted case of yellow fever in the whole West Indian squadron," a well-marked instance occurred in the "Barraconta," in the person of a boy who had been exposed for an hour about noon, at the mast-head, to the sun's rays directly upon him.

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[1] Report for 1856, p. 89; for 1857, p. 42; for 1860, p. 70; &c. &c.

[2] In relation to this diagnostic difficulty, the narratives of the surgeon and of the assistant-surgeon of the "Bristol" are specially worthy of attention, as all the incidents connected with the memorable outbreak in that ship are detailed with great minuteness and ability. These narratives reflect great credit on Mr. Ayre and Mr. Buckle. The observations on the same subject by the surgeon of the "Cadmus," in which yellow fever prevailed to some extent in the same year, at Port Royal, also deserves special notice.

"He was seized on the following morning with a violent headache, and was in a delirious or unconscious state during the whole period of the attack. There was yellowness of the conjunctiva; and for some hours before death a black grumous fluid welled out of the mouth without effort. Immediately after death decomposition set in, the body rapidly inflated, the skin became black, the extremities mottled, and the face of a dirty-yellow colour. This appears to have been a case of malignant fever induced by insolation."

This was the only death from fever on board the ship, throughout the year. A solitary fatal case occurred the same year in the "Buzzard," in which death rapidly supervened, and of which Dr. McKay remarks that—

"The symptoms unquestionably resemble those of specific yellow fever. The only circumstance that threw a doubt upon the case is the fact that it did not spread among the ship's company, and that other officers and men equally exposed did not contract it. * * * It is more than probable that it was an example of that malignant form of remittent fever which is accompanied by yellowness of the skin and black vomit."

The two fatal cases in the "Handy," in 1864, were of the same sort; and so with those in some other ships.

The history of the results of removing yellow fever patients from infected ships to a healthy hospital on shore serves to cast much light on the character and attributes of the disease. Hitherto, the want of full information from the records of our naval hospitals, abroad and at home, has been one of the principal *desiderata* in the annual reports. It is owing to this want chiefly that we cannot follow out this part of the subject as we could wish. The results in Jamaica, in 1860, as we have seen, were eminently satisfactory; and the same seems to have been the case in 1856, a most sickly year, when the fever was very prevalent on shore at Port Royal and at Kingston. Of nearly 100 cases admitted from the "Malacca," the "Termagant," the "Hermes," and other ships, 22 were fatal. "Five patients labouring under other diseases in the hospital, who occupied a ward separated from the yellow fever cases, were attacked, and one died." The medical and other attendants escaped; nor do we observe mention made of any casualties among the staff of this hospital in any of the reports, in other years, although not a few deaths among the medical officers have occurred on board infected ships.

In the hospital at Bermuda, too, the reception of yellow fever patients from infected ships appears to have been attended with the happiest effects as regards the sick, and without danger to others. In the case of the "Argus," in 1856, when the disease all but ceased at once after the crew, sick and well, had been landed, the surgeon remarks that "not one of the attendants at the hospital on shore was attacked, whilst nearly all those who attended the sick on board became the subjects of the fever, showing that the morbific poison on board had become more detrimental to health than the personal emanations from the sick, at least in a well-ventilated hospital." The historical sketch of Bermuda epidemics by Deputy-Inspector Dr. Smart, in the first volume of the 'Epidemiological Transactions,' relates some striking proofs of the utility of landing the sick from infected vessels in suitable hospitals.

In the numerous instances of late years where crews, sick and well, have been landed at the island of Ascension, the disease seems to have speedily much abated; and in no instance to have extended to the garrison and other residents, always provided that direct communication with the infected ships was prevented.

The experience of the Marine Hospital on Staten Island, in the harbour of New York, has shown how small is the risk of cases of yellow fever removed from infected ships into airy wards on shore spreading the disease. Not a single instance of this casualty had occurred for many years, although several hundreds of yellow fever patients had been received from vessels in quarantine.[1] A good many seizures, however, had taken place among persons whose duties, or curiosity, had led them on board these vessels. Like results were observed at Lisbon, in 1857, among the refugees from the infected districts:

> "Among those who fled, numbers were already incubating the disease, and of course died with it in their respective places of refuge, more or less distant from Lisbon. In 182 cases of this description, the Portuguese Government caused inquiry to be made, whether persons about the sick had shown any signs of the infection; and the answers were that in no single case had this occurred."—*Eighth Report of the Medical Officer of the Privy Council.*

Within the last ten or twelve years cases of yellow fever have, on more than one occasion, been landed from ships of war in Plymouth and Haslar Hospitals, without any but good results. In 1857, three cases were landed from the "Orion."

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[1] It was the consideration of this fact, and of other similar evidence, that led the National Quarantine and Sanitary Convention of the United States, at their meeting in New York in 1859, to adopt a resolution "that, in the absence of any evidence establishing the conclusion that yellow fever has ever been conveyed by one person to another (when removed from an infected ship to a well-ordered hospital, *Rev.*), it is the opinion of this Convention that the personal quarantine of cases of yellow fever may be safely abolished, provided that *fomites* of every kind be rigidly restricted." Of 74 medical men present, 70 voted for it.

The neutralising and controlling effects of free ventilation around yellow fever patients, on board ship, is strikingly shown by many notices in these reports. Besides the cases of the "Bristol" and of the "Barraconta," already mentioned, reference may be made to the cases of the "Rifleman" and "Wasp," in 1857; of the "Styx" and "Siren," in 1858; and of the "Sharpshooter" and "Alert," in 1859. In most of these vessels, the practice of treating all the yellow fever patients on the upper deck under a canvas awning was adopted, and apparently with excellent results in arresting the persistence and spread of the disease among the crew. Dr. Bryson highly approves of the practice. Speaking of the "Rifleman," he says:—"These measures are most creditable to the commanding and medical officers of the ship, and they contrast strongly with those adopted in other vessels, in which no precaution whatever was taken to prevent the disease spreading." The reader of the 'Navy Reports' cannot be surprised at the rebuke contained in these last words; for nothing will strike him more forcibly, in perusing the accounts of the disastrous ravages in so many of our ships of war of recent years, than the total absence of all reference to the use of any special sanitary and hygienic precautions in sickly seasons, and prior to the development of the disease on board, as well as during its actual prevalence. The mephitic condition of the between-decks in several of these vessels, and the full state of their holds, as discovered on their arrival at their port of disembarkation, are anything but creditable to a public service. It is scarcely necessary to say that many outbreaks of enteric fever on board ship have been clearly traced to these intrinsic causes of mischief, and that remittent and yellow fevers are invariably more frequent and severe when such evils exist. Unhappily this is by no means a rare occurrence, even in the present day, and especially in steamers, in which an accumulation of offensive rubbish under the furnace, &c., is extremely apt to take place. Whether the accumulation of coal on board has any injurious influence on health, or in predisposing to attacks of certain fevers, as has been conjectured, it is impossible at present to determine. That collier vessels suffer more severely from yellow fever in the West Indies than any other vessels is a frequent remark; but then these vessels are almost always in the worst possible sanitary and hygienic conditions. There is another element of sickness on board steamers which must be taken into account, in comparing the health-state of the navy now with what it was when every ship was a sailing one, and when there was no artificial heat on board save that of the galley.

It is an attribute of yellow fever that its development and malignancy are promoted by a high temperature, and mitigated and controlled by a low one. It ceases to manifest itself when the thermometer falls to 60° or 55°. The season in which it is, very generally, most prevalent and fatal is between July and October. Almost all the severe outbreaks in our ships of war have occurred within this period, which is moreover the usual sickly season in tropical countries, and that, too, when enteric as well as remittent fevers, along with fluxes and cholera, are most frequent everywhere. The fact is significant in a preventive point of view; for it obviously suggests the need for extra sanitary precautions when a danger is to be apprehended. So marked is the arresting influence of a reduced temperature on the activity of the yellow fever poison, that the Admiralty have been strongly urged to order that all ships of war attacked with the disease should immediately leave the tropics, and run as quickly as possible to the northward, either to Halifax or Newfoundland, or to England. In several instances when this measure has been adopted without due attention, at the same time, to maintain thorough ventilation and other indispensable precautions on board, the fever has continued to rage for ten or twelve days before the destination was reached. The exigencies of the service must, of course, often prevent a ship leaving a station upon the appearance of the first cases; and fortunately the step is often unnecessary; as was the case with the "Bristol," and other ships on the African station, which, by running to the southward so as to reach Ascension, have lost all traces of the disease, and continued healthy afterwards. It would, moreover, be a terrible thing for our merchant seamen if experience proved that "there is no safety for a ship's company amongst whom yellow fever exists but in the rapid flight of the vessel to a colder region."

And now what do the foregoing considerations lead to? The doctrine, which has of recent years been so urgently insisted upon, that yellow fever is primarily and essentially a highly contagious specific disease, just like smallpox or scarlatina, arising and spreading as these exanthemata do, and that its development is little, if at all, connected with any conditions of local insalubrity, is scarcely in accord with the evidence adduced. Nor does it agree with the results of observation, either in the medical service of the army, as detailed in the military reports, or in the practice of our colonial medical brethren. That the fever, as Sir G. Blane asserted,[1] is apt to manifest under certain conditions unmistakable contagious properties cannot, we think, be fairly disputed by any unbiassed person. But to maintain that every case of the disease is due to a poisonous emanation from a previous patient, and that this human poison must have in some way or other, directly or indirectly, reached the new sufferer, is, to say the least, purely conjectural. It will be observed by the reader of the 'Navy Reports' that the origin of the first case or two in a ship is generally obscure, and that the disease is seldom, if ever, traced to contact or proximity with sick persons, although it is frequently attributed to landing in a place where the fever is prevailing. A boat's crew go ashore at Havanna or St. Thomas, two of the worst fever hot-beds in the West Indies, in both which the harbours and foreshores are reeking with the foulest abominations, and there loiter about the streets or resort to a public-house for drink; and, a few days afterwards, one or more of the men sicken with what turns out to be yellow fever. On the one hand, the surgeon and the captain of the ship ascribe the sickness "to the filthy polluted state of the water in the harbour of Havanna, and to the peculiar state of the climate;" and the medical officers of the Admiral's ship, to whom the question was submitted for their opinion as to its probable cause, declare that "there is no positive evidence that the disease was introduced by direct contagion." On the other hand, the authorities at home assert that "it is not necessary to look to the offensive state of the harbour, or the smell of bilge water on board ship, as a cause for so specific a disease as yellow fever when it was prevailing on shore;" and that "it is worse than illogical to talk of this fever being attributable to local causes." No wonder that the Admiral was perplexed what to believe about the matter. As long as such discordancy exists among the medical men of a service, who can expect that professional opinion can have much weight either with the executive, or with the public in general?

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[1] "I saw enough in the hospital at Barbadoes, and in the ships and hospital at Jamaica, to convince me of its contagious nature in certain circumstances; and I remain persuaded that, whenever it is so aggravated as to appear in an epidemic and pestilential form, it is truly contagious."

As a consequence of extreme contagionistic doctrines, strict quarantine is declared to be the only trustworthy safeguard against the introduction of the disease, either into vessels from the shore, or from infected vessels into a healthy place on the land. "Localities still exempt from the disease may be insured an immunity from it by the enforcement of stringent quarantine;" and it is confidently maintained that "had quarantine been as rigidly enforced at Bermuda in 1864 as it was at New Orleans in 1862, when yellow fever was prevailing as an epidemic within a short distance of the mouths of the Mississippi on which that city stands, there can be little doubt that the island would have escaped the disastrous visitation it sustained." The soundness of this conclusion, as well as the accuracy of the statement on which it rests, is, it is generally believed, most questionable. Might it not be better that all authoritative declarations of opinion on such contested matters were avoided in official reports on the public service? Their great object is to furnish a record of well-ascertained facts, and of faithful narrations of events, so that the reader may judge for himself from the evidence. That the consideration of the question of quarantine, in relation to yellow fever, is one of much interest and importance is well known to medical officers of the navy; and none are perhaps so capable of judging correctly on the subject, or of guiding public opinion upon it, as many of these gentlemen. And much need there is at the present time in this country, no less than abroad, that our own Government be rightly instructed and advised as to what measures are necessary, and what are unnecessary, for preventing the importation of foreign pestilences by shipping, after the proceedings adopted by the quarantine authorities at Southampton towards the mail steamers from the West Indies, during the last two winters. Passengers, among whom there had not been a trace of sickness during the voyage, were detained in strict isolation at the Motherbank when the temperature was near the freezing point; nothing was allowed to be landed from the ship (which was guarded by two gunboats to prevent communication with the shore) except specie and the letter-bags; and even the paper containing the replies of the surgeon and captain to the queries of the quarantine officer was not returned to them for signature, for fear of its being a vehicle of infection. When such absurdities are being committed, it is surely high time that the opinion of experienced medical officers of the navy, and of other competent physicians, should be ascertained by the government authorities who have to deal with the question.

In concluding our remarks, we heartily congratulate the Director-General and Dr. Mackay on the success of their exertions in increasing the value of the Reports from year to year. Not only have these been greatly expanded so as nearly to double their former size, and thus to afford ampler space for much important information respecting the health of the navy, but they have been brought up to within little more than a year and a half of the present time. This has been effected by the issue of the two volumes, for 1865 and 1866, in the course of this year, for which the service and the profession are indebted to Dr. Mackay's untiring energy. The opportunity now given to the medical officers to distinguish themselves by the value of their communications, so that these may be noticed and find a place in these annual reports, will, doubtless, stimulate many to win this honourable distinction, and will moreover serve to bring the service into more favour with the profession than it has been of late years. It is urgently necessary that the influence of enlightened medical opinion be more and more felt in the administration of the navy in all matters relating to health; for costly blunders still continue to be committed in the construction and arrangement of our ships of war which seriously injure the efficiency of the crew, and which might be easily avoided, if every ship was thoroughly examined by a sanitary officer before she was commissioned. One of our iron-clads, the "Royal Oak," was found to be a most unhealthy vessel from first going to sea, and thrice had she to be inspected by a sanitary board in the course of twelve months, and great changes to be made in her deck arrangements before her high sick-rate was reduced. And this is but one of many similar instances that might be cited.

REVIEW III.

Des Bains d'Air Comprimé. Court Aperçu de leurs effêts physiologiques et therapeutiques, précédé d'une description de l'établissement Médico-pneumatique de Stockholm, par Oskar Th. Sandahl, Docteur en Médecine et en Chirurgie, Professeur Agrégé à l'Ecole de Médecine de Stockholm, 1867. Pp. 60.

Baths of Condensed Air.—A short account of their physiological and therapeutic effects, preceded by a description of the Medico-pneumatic establishment of Stockholm. By Dr. O. T. SANDAHL.

IT is nearly thirty-five years ago since M. Emile Tabarie, of Montpellier, presented to the Institute of France his first memoir on the effects of compressed air upon the human system. That publication was followed by others by the same author and by Messrs. Junod, Pravaz and Milliet of Lyons, Bertin, Vivenot, Lange, Panum, the author of the present treatise, and numerous others, to the number of thirty or more. At the present moment establishments adapted to apply the system introduced by M. Tabarie exist at Montpellier, Lyons, Nice, Stockholm, Upsala, Gothenburg, Helsingfors, Copenhagen, Berlin, Dresden, and various other European cities, superintended in many cases by men of unquestionable scientific and professional position who have sedulously investigated in their own persons as well as in those of their patients both the physiological effects and the therapeutic efficacy of this novel physical agency. It can, therefore, hardly be considered premature if we endeavour to present to our readers a brief account of the results which have been thus obtained, so that they may be able to judge for themselves whether some claim has not been made out for a somewhat better treatment of it in this country than the almost contemptuous indifference with which it has been hitherto received by the medical profession. In so doing, we shall avail ourselves freely of Dr. Sandahl's *brochure*, which is largely founded on the researches of his predecessors in the subject.

But before entering upon the strictly medical aspect of the question, it may not be amiss to devote a few words to explain the nature of the means which are employed in this system of treatment by compressed air, so that should any of our readers feel disposed to enter personally upon a course of experimentation, they may be able to form some idea of the stock in trade with which they will have to provide themselves. Any one who has seen an ordinary diving bell, such as is exhibited in the London Polytechnic Institution for the delectation of visitors, will have no difficulty in representing to himself the little bell-shaped chamber, constructed of stout iron plate, about six feet in diameter and eight in height, into which the would-be inhaler of condensed air is introduced on first commencing the treatment. Into this chamber, which is lighted and provided with the resources necessary to enable a couple of hours to be comfortably whiled away, air is driven by a steam-engine at a pressure which does not ordinarily exceed half an atmosphere, having been previously warmed or cooled to a required temperature, and purified from impurities by filtration through an apparatus contrived for the purpose. The duration of a *séance* is, as we have intimated, ordinarily two hours, during the first and last half-hour of which the pressure of the internal atmosphere is gradually adjusted; and this is the only part of the process which can in the slightest degree inconvenience the patient. In such a chamber, then, an observer has abundant opportunity of examining carefully the effect of the increased atmosphere pressure either upon himself, or on another person, as has been done by Lange and Vivenot on patients under treatment, and also by Francois and Foley on workmen employed in some large caissons used in certain works connected with the construction of bridges on the Rhine and at Argenteuil. In the first place, such an observer would note a diminution in the rapidity of the movements both of the respiration and the circulation; the former falling, on an average, three, and the latter ten per minute; the effect upon the pulse being also very perceptible in the altered form of the fluctuations of the sphygmographic line to which it gives rise. Moreover, it would be observed that not only the frequency but the rhythm of the respiratory movements was affected, the ratio of the inspiratory to the expiratory murmur being diminished from 4—5 to 4—7 or even 10. Competent observations also by the stethoscope and the spirometer, seem further to show that the decrease in the rate of the movements of respiration is accompanied by an increase in the volume of the inspired air, to the extent of 3 or 4 per cent., and this increase is stated to be somewhat permanent in its nature, being evident for some time after the patient has been exposed to the influence of the condensed air. With the increase in the volume of the inspired air, there would be noted, according to Lange, an augmentation in the force of the respiratory movements, that is to say, in the force with which air is drawn into and expelled from the lungs by the respiratory muscles. This fact, too, is closely related to another, which from the evidence of the same observer, appears to be sufficiently established as a result of the inhalation of condensed air, and that is, an increase in the muscular energy of the body generally. This was determined by Lange by means of a number of careful experiments on a young man whose power of lifting a given weight was tested by a series of observations made under ordinary and increased atmospheric pressure respectively; when it was found that under the latter condition the lifting power was increased on an average about 5 per cent. This phenomenon is quite in accordance with the opposite effect of a rarified atmosphere in depressing the muscular energy of the body, which is familiar to all who have made the ascent of lofty mountains, and which is particularly described by Humboldt and Boupland in their account of the ascent of some of the Cordilleras.

These, then, are the physical results which the continued inhalation of condensed air produces upon the system, and with respect to which the general agreement of competent observers leaves no reasonable doubt. It could scarcely be expected that such results could be obtained without some indications being observable of an alteration in the chemical relations of the body. The most marked of these is the *increase of carbonic acid in the expired air.* Vivenot found that the air of a complete expiration under condensed air contained on an average fifty milligrammes more of carbonic acid than under ordinary atmospheric pressure, and the experiments of Lange give a closely concordant result. It has been seen that the absolute volume of air expired under the influence of a condensed atmosphere is increased; and it might at first sight be thought that the augmentation in the amount of expired carbonic acid was due to this cause; but a comparison of the ratios of the two elements shows that the excess of carbonic acid bears, on an average, to the excess of air the ratio of six to one, *i.e.*, that the amount of carbonic acid is increased five times beyond the amount which would be due simply to the increase in the volume of the expired air. This is an important and interesting fact whether we look upon it from a physical or a physiological point of view. In the former case, we are presented with a confirmation of the theory as to the nature of the process of respiration which has been maintained by Vierordt and Valentin, founded on the law of Graham, viz., that the elimination of the carbonic acid from the blood in the lungs, no less than the absorption of oxygen, is a purely physical process, governed by the ordinary laws of transfusion, and dependent simply upon the difference between the density of the external air and that of the gases in the blood; in opposition to the view of Funke, who maintains that these laws are not sufficient by themselves to account for the phenomena of respiration, and that the chemical affinity which exists between the oxygen of the external air and the constituents of the blood exercises an important influence upon the process. The physiological value of this fact is even greater, for the increase in the amount of exhaled carbonic acid must of course be dependent upon an increase in the activity of the chemical changes which take place in the body, that is to say, in the oxydation of the carbonaceous elements of the blood and tissues. Such a result, easily producible artificially, and capable of moderation at will, cannot but be regarded by the physician as supplying him with a therapeutic agency of the highest value for certain purposes, and we consider that scientific medicine is much indebted to Vivenot and Lange for having clearly established so interesting and practical a fact. It would seem a necessary inference, and indeed it is accepted as such by those who, like Vivenot, have practically investigated the subject, that the increased excretion of carbonic acid in the expired air implies an increased absorption of oxygen; but this fact though assumed has not yet been chemically demonstrated. Its probability is shown, however, not only by general chemical considerations, but also by a circumstance which has been noted by Foley and others, that the venous blood of workmen who have been immersed for some time in the condensed atmosphere of caissons approaches very nearly in tint to that of the arteries. Moreover, additional evidence in the same direction is supplied by other observations of Vivenot, which show that the temperature of the body rises under the influence of the condensed air, attains a maximum coincidently with the maximum of pressure, and falls with the resumption of the ordinary pressure even to a lower point than it exhibited at the outset. It is true that Vivenot ascribes a portion of this result to the diminished evaporation from the surface of the body which takes place under the higher pressure, but by far the larger share of it must undoubtedly be ascribed to the additional amount of oxygen absorbed. Although observers are unanimous with regard to the effects thus produced by the inhalation of condensed air upon the chemical elements of the respiratory function, they are not, unfortunately, agreed upon the length of the period for which this effect lasts, Vivenot and Sandahl asserting that it remains for some hours after the bath, whilst Professor Panum of Copenhagen, who has investigated with great care the whole subject, states that this, as well as the other effects of the bath, does not persist beyond the duration of the bath itself.

Amongst the results of the bath to which we have not as yet referred, are two which deserve notice, both for their intrinsic importance and for their relation to the question to which we have just alluded: they are, an *increased activity of the digestive functions*, and an *augmentation in the amount of the urinary excretion.* All observers agree in their testimony as to the improvement which takes place in the appetite of patients whilst under treatment by the compressed air-bath, Sandahl further asserting, as the result of numerous experiments, that the first and immediate effect of the bath is to produce a diminution in the weight of the body, which is, after a time, followed by as marked an increase. The phenomena which are observable in the urine are, increase in the amount both of its solid and liquid elements, marked increase in the amount of urea, and also to some extent of sulphates, but diminution of phosphates; in some cases, indeed, these latter salts completely disappear, but as it has not yet been shown whether they pass off by the bowels, or not, it must remain an open question whether this is the true explanation of their diminution in the urine, or whether they are retained in the economy for histogenetic purposes.

The most marked effects of a condensed atmosphere upon the animal economy would thus appear to be of a physiological character, and ultimately referable in all probability to the increased absorption of oxygen, and to the histolytic changes to which it gives rise. It is therefore rather strange, that on turning to the therapeutic results which are stated to have been obtained by an application of the process to various morbid conditions, it is to the mere physical influence of pressure exerted upon the capillary vessels of the general surface of the body that we must look for an explanation of the beneficial effects which are said to follow its employment. Thus, in acute and chronic bronchitis, and in chronic laryngitis, especially when accompanied by obstruction of the Eustachian tube, and consequent deafness, the improvement which the statistics of various authorities show to have been effected in a very considerable percentage of the cases submitted to treatment, would appear to be, in all probability, attributable to the tonic influence of the condensed air upon the relaxed and congested capillaries of the mucous membrane, rather than to any other cause. Amongst the affections in which the compressed air-bath has been employed with the greatest success by Dr. Sandahl is hooping-cough. Although this is eminently one of those affections which from experience differs considerably in intensity and amenability to treatment according to the character of the epidemic, a percentage of 88 completely cured out of 102 patients treated, and that, too, during a course of baths extending, at the outside, to three weeks, and in many cases, not beyond nine or ten applications, is too satisfactory to be capable of being explained away by any supposition of accidentally favorable circumstances, or even to be parallelled by any other mode of treatment. Whether the result in this case is due to the increased oxygenation of the blood, or, as we are rather inclined to think, to the locally tonic influence of the condensed air upon the irritable nerves and congested vessels of of the bronchial membrane, must be left an open question. If the former be the correct explanation, it is certainly rather in antagonism with a mode of treatment which has latterly acquired some repute in the chronic stage of hooping-cough, viz. the inhalation by the patient of the ammoniacal atmosphere of the purifying chambers in gas works, in which there is undoubtedly no excess of oxygen, but rather the reverse.

Although it is scarcely to be expected that chronic pulmonary emphysema could be more than temporarily alleviated by this or any other mode of treatment, both Sandahl and Lange assert that they have seen such improvement follow an extended course of condensed air-baths, even when there was reason to believe that emphysema had existed for some years, as practically almost to amount to a cure. The evidence on this point, however, is not quite so satisfactory as might be wished, nor is the author's statement that "so long as emphysema consists only in vesicular ectasis of a more or less extensive character, it is invariably cured by compressed air, which restores to the pulmonary tissue its elasticity and contractility," beyond the possibility of cavil. Under these circumstances we must wait for a fuller trial of the process in this affection before forming a positive opinion on its utility. So, too, in the case of pulmonary tuberculosis, although the statements of Storck, Lange, Panum, and the author, are all very decided as to the benefit which they have seen produced in the early stage of the disease, before much destruction of lung tissue has occurred, by a course of compressed air-baths, we must confess that we should have felt better satisfied by the details of a fair number of cases which were assumed to have been benefited by by the treatment, so that an opportunity might be given to the profession in general of estimating for themselves how much was due to the remedy in question, and how much to other favorable conditions in which the patient might have been placed. At the same time we are bound to say that the tone of Dr. Sandahl's brochure is such, and the spirit in which it is written is so different from that in which too many treatises of a similar kind are conceived, that we cannot in justice refuse to attach considerable weight to the statements of his own experience, which he gives, even when it is unsupported by actual facts. There is one more class of affections to which we have referred, in which the employment of the compressed air appears to be eminently useful, and to which the preceding remarks do not apply, and that is certain uterine affections, more particularly those connected with arrest of the menstrual functions. With reference to this class of affections, Dr. Sandahl submits a series of cases, which seem to show that the compressed air-bath has a very potent influence in stimulating the reproduction of the catamenial function when it has been arrested by constitutional causes. Out of thirty-two cases of this kind, the result of the treatment in twenty-eight was entirely favorable, and in three of the others the reasons of ill-success became sufficiently apparent afterwards.

We have thus endeavoured to lay before our readers a brief summary of the physiological effects and therapeutic uses of a physical agency to which we think that too little attention has hitherto been given in this country. That the compressed air-bath has been introduced into England we are aware, but this has scarcely been done under such auspices as were likely to ensure for it the confidence which was requisite to give it a fair trial. Unfortunately, too, it is a mode of treatment which, from the costliness of the apparatus required to put it in practice, can only be adopted in large establishments, such as those at some of our watering places, or as public hospitals. In the cause of science, and we may say also of humanity, it is desirable that its value should be tested in a way to leave no doubt as to the trustworthiness of the results attributed to it, and this can only be done in a hospital where there are no private interests to distort, either wilfully or otherwise, the evidence which its use may supply. We scarcely think that either our large general hospitals, or those which have been specially founded for the treatment of diseases of the chest, could do a greater service to the cause of medicine just at present than by erecting a bath for the trial of the system, and placing the results before the profession, when sufficient evidence may have been accumulated to allow of an impartial opinion on the subject being formed. For ourselves we have no doubt whatever that the result would be a satisfactory one, and that it would lead to the bath being introduced into the recognised list of therapeutic agencies—not as a panacea for all sorts and kinds of disease, as some of its advocates have rather unwisely or dishonestly represented it to be, but as a means of treatment in certain morbid conditions, which is easy of application, intelligible in its physiological action, and harmless at the worst. We really think that it is time, after it has been upwards of thirty years before the profession, and when its value is recognised by men of undoubted scientific standing in all parts of Europe, that it should at least have the benefit of a fair trial in this country. Any one who wishes to become more fully acquainted with the subject cannot do better than provide himself with Dr. Sandahl's treatise, which discusses the merits of the system of treatment by condensed air with the moderation which a true man of science might be expected to show.

REVIEW IV.

Leçons de Clinique Médicale, faites à l'Hôpital de la Charité. Par. S. JACCOUD, Professeur agrégé à la faculté de Médecine de Paris, &c. Paris, 1867. Pp. 865.

Clinical Lectures on Medicine. Delivered at the Charité Hospital by Professor S. JACCOUD. Paris, 1867.

THE publication of clinical lectures is to be justified by the expectation that they will interest a wider circle than those to whom they were originally addressed, and this mainly by their concentrating the valuable experience of a long professional career.

To mention no living examples, Graves and Trousseau have in our own time selected this medium for communicating to the medical world their priceless additions to its knowledge; and their pupils might have fairly echoed Alexander's selfish complaint that their masters were thus depriving them of an advantage they had until then exclusively possessed.

Or again, a lecturer with a smaller store of personal experience may so collect and condense the latest discoveries in practical medicine as to produce a work which will be, perhaps, even more useful to the practitioner than to the student, as showing the improvements effected in our restless, ever-moving science; and this is the principal merit of the lectures before us. Many personal observations of no small importance are, indeed, scattered through the book, some of which we shall quote hereafter; but its most prominent character is that it thoroughly represents the state of science as to the subjects with which it deals. Professor Jaccoud is already favorably known in France as the author of detached monographs on albuminuria, paraplegia, and ataxia; while, having studied in Germany, and being well acquainted with English, he has the advantage—too rare in France—of being able to appreciate the progress of medicine in other countries besides his own. His knowledge of English is evidenced by the excellent translation which he has made of Graves's clinical lectures; and as for German, the French reviewers of his previous works seem to look upon him as belonging to that country rather than to their own—a criticism which the volume before us substantiates in every point save one; there is a clearness and neatness of style which are rather uncommon out of France.

The most cursory reader of these lectures cannot fail to be struck by the great number of thermometrical observations in many acute diseases, which Professor Jaccoud has collected from various German monographs, and from his own hospital wards; and the thing he most frequently urges upon his students is the great value of the thermometer in practice as a help to diagnosis and prognosis. Although some very valuable contributions to this department of medicine have proceeded from English physicians,[1] it is, on the whole, too little known in this country; and we think, therefore, we shall be doing good service in presenting our readers with an abstract of some of the passages in the volume before us which bear upon it.

Some of the Germans, whose observations Jaccoud makes use of, have been in the habit of taking the temperature five or six times a day. This is, however, impossible, and unnecessary in practice; and Jaccoud has contented himself with using the thermometer twice a day—at about eight in the morning and six in the evening; it is placed in the axilla, and left there for twenty minutes, in order to be quite certain that the highest point is reached.[2]

[1] Not to mention the numerous researches on the temperature of acute febrile diseases, Dr. Clouston has quite recently shown ('Journal of Mental Science,' April, 1868) that this mode of investigation is of great value in the diagnosis of some of the most obscure cases of cerebral disease.

[2] We may add, that we have throughout, in translating centigrade into Fahrenheit degrees, made the nearest approximation possible without going beyond one decimal.

In *acute pneumonia*, the variations of temperature have been examined in several thousand cases, chiefly by Wunderlich, Traube, Baerensprung, and Thomas, of Leipzig; and they are found to follow a strictly uniform course. Immediately after the initial rigor, the temperature begins to rise, and continues to increase for about forty-eight hours rapidly, and with no further interruption than a fall of about half a degree each morning. So soon as four hours after the rigor, Ziemmssen has noted a temperature of 102·5°, which, eight hours later, had risen to 104·4°, and still higher temperatures have been observed at an early period of the disease; thus Thomas has met with a temperature of 104·9 nine hours after the first symptoms. At the end of the second day the thermometer has almost invariably reached its highest point, which is ordinarily somewhere between 104° and 106°, and the second period of "status" begins. During this stage the daily variations of temperature have been carefully studied by Thomas, and he has shown that they are generally from 1° to 2°, occasionally even more. It is very rare to meet with so small a variation as four or five tenths of a degree, and equally so to observe the opposite extreme of a decided intermission. Jaccoud's examination of a large number of cases has led him to the general conclusion that this stage ends, in one fifth of the cases, from the third to the fifth day; in three fifths from the fifth to the seventh day; while in the remaining fifth it is continued until after the seventh day, usually to the eighth or ninth. Its termination is sometimes announced by a decided oscillation of the thermometer, either upwards or downwards; but usually, without any such warning, the thermometer is observed to fall with such rapidity, that in thirty-six or forty-eight hours it has reached its normal position, and the process of "defervescence" (as it has been happily termed by Traube) is finished. This great celerity of both invasion and decline is characteristic of pneumonia, and the latter agrees with what we know, from other sources, of the speedy return of the disturbed functions of nutrition (as evidenced by loss of weight, and by the altered urinary secretion) to their natural state. The pulse generally falls "pari passu" with the temperature; but in rare cases it takes a longer time in doing so, the thermometer being then the better indicator of the improvement which has occurred.

The almost invariable uniformity of these changes of temperature in pneumonia is rightly insisted upon by Jaccoud as one of the strongest proofs that it is a disease which runs a certain definite course, and, in ordinary cases, tends to recovery. This has an important bearing on the treatment recommended by him, of which we shall presently have something to say.

The invasion fever of *variola* closely resembles that of pneumonia, in the rapid and continual increase of the temperature until the first appearance of the eruption, an advance of three or even five degrees in the first twenty-four hours being by no means unusual. The highest temperature yet noticed at this moment, 106·7°, was observed in the patient who formed the subject of Jaccoud's lecture. If the case be one of varioloïd or discrete variola, the temperature falls at once, and even more rapidly than it has risen; in thirty-six or forty-eight hours it has returned to the normal point, whence it generally, but not always, continues to fall to as much as one and a half or two degrees *below* par. In the confluent form of the disease defervescence takes place more slowly; it begins, on an average, on the evening of the fourth day, and takes forty-eight or sixty hours to reach a point usually a little *above* par. The secondary fever begins on the seventh, or more rarely on the sixth, day of the disease, consequently after a short respite when the form is discrete, and almost immediately when it is confluent; the temperature generally continues to rise for twenty-four hours or more; its course is somewhat irregular, but is, on the whole, remittent in the discrete, and nearly continuous in the confluent form, lasting four or five days in the former case, and eight or ten in the latter. The thermometer has never hitherto been observed to rise above 104°, and is always lower than it was during the invasion stage. To judge from the three cases mentioned by Jaccoud, the secondary fever is entirely absent in varioloid; but further observations are necessary to show if this is the rule.

Leo discovered, in the course of his observations, during an epidemic of small-pox at Leipzig in 1864, that in confluent variola another "tertiary" febrile movement occurs at the time of desiccation. The maximum noted was 104°, and the duration was about twenty-four hours.

In fatal cases the temperature generally continues to rise until the end. In one of Leo's patient's, who died of confluent variola on the tenth day, the secondary fever, instead of abating, steadily increased, until shortly before death the thermometer was at the very unusual height of 109°.

The thermic variations of *typhoïd* fever are better known in this country than those of any other disease; still, it may be worth while to give the general conclusions deduced by Jaccoud from an analysis of 1400 cases (700 of Wunderlich's, 500 of Griesinger's, and 200 of Thomas's).

Like pneumonia and variola, the febrile cycle of typhoïd may, for our purpose, be divided into three stages of ascent, status, and decline.

During the period of ascent the temperature gradually rises for four or five days, the average increase each day being 2·7°, but as a fall of 0·9° is noticed every morning, the *net* advance in twenty-four hours is only 1·8°.[1] These figures, of course, vary in individual cases, but the mode of progress is constant, supposing no active treatment has been employed, and is highly characteristic of the disease.

In the period of status the thermometer, having reached a maximum of about 105°, ceases to rise, and oscillates about that point, with a daily variation of from 0·2° to 1°. The remissions still occur in the morning, and, as Thomas has shown, become greater after the tenth day.

This ushers in the stage of decline, which is the reverse of the first period, the thermometer rising somewhat every evening, but falling considerably in the morning, until the normal state is reached. The duration of these two latter stages is very variable; that of status lasts from nine to twenty-two days, that of decline from seven to twenty-one days.

Such is the ordinary and regular course of typhoïd fever; but it frequently undergoes modifications of great importance. Thus, a very decided remission, generally of more than two degrees, and less than three, occurs, in a great many cases, at the end of the first week. (Wunderlich, who first called attention to this, fixed it for the seventh day of the disease, but Jaccoud points out that it quite as frequently occurs on the sixth or eighth.) It lasts only a few hours, and has no apparent bearing on prognosis, but is of importance, because, if ignorant of its frequent occurrence, the physician may doubt the correctness of his diagnosis, which, in reality, it strongly confirms.

Again, a considerable irregularity, occurring about the fifteenth day—Wunderlich's "stadium amphibolum"—is a very bad sign, and will justify a grave prognosis, even supposing all other symptoms favorable.

In fatal cases the temperature rises, as in other acute diseases, until death, the highest point hitherto noted being 108·2°; but when the patient dies of intestinal hæmorrhage this rule is not observed. On the contrary, loss of blood produces a sudden and very considerable diminution of the temperature; severe

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[1] Perhaps the following ideally-regular case, given by Jaccoud, will explain the statement in the text:

First evening	temp. 100·4°
Second morning ...	„ 99·5°
Second evening ...	„ 102·2°
Third morning......	temp. 101·3°
Third evening	„ 104·0°
Fourth morning ...	„ 103·1°
Fourth evening ...	„ 105·8°

diarrhœa has the same effect to a less extent. Secondary inflammations may cause the thermometer to rise for a day or two, or may delay the stage of decline; but their influence is only temporary, although they last longer.

As to therapeutical agents, emetics, purges, digitalis, antimony, quinine, and calomel in non-purgative doses; all lower the temperature, but in no way affect the duration or progress of the disease. The period of invasion is so very characteristic as to be of the greatest importance in diagnosis. Thus, a patient in whom a temperature of 104° is noted on the first or second day, or one in whom, on the fourth or fifth day, the temperature is not above 102°, has not typhoïd fever.

We have, as yet, no observations of the way in which *acute tuberculosis*, in its early stages, affects the thermometer, but to compare the two diseases when fully established, we find it is far more irregular than typhoïd; the maximum varies from day to day, and the morning remissions are very much greater, the thermometer often being two and a half or three degrees lower than the night before.

As regards prognosis, a temperature of 108·5° is invariably mortal; 107·5° is almost always so. The lower the average maximum is during the second stage the less serious is the prognosis; thus, an average maximum of 106° is always serious, 104° is ordinary, and 103° is favorable. The more decided the morning remission, the better the case; on the other hand, it is of bad augury that the daily exacerbation should begin before noon, and last until after midnight. Gradual decline of the temperature during the period of status is favorable, but a sudden fall to 97° or 98° indicates fatal collapse or hæmorrhage. A sudden elevation of temperature is also a sign of nearly approaching death. Unfortunately, the first period, so useful in diagnosis, furnishes no clue whatever to the future course of the disease. But the existence of all its characters in those puzzling cases where, the fever terminates in two or three weeks or less, strongly confirms the opinion of Griesinger and Lebert, that these are cases of "aborted" typhoïd, and not of some distinct morbid species.

To return to the subject of pneumonia. Several other points of interest about the disease, besides the thermic variations, are discussed in the three lectures devoted to it. Professor Jaccoud's personal experience of inflammation of the lung enables him to tell us that the redness over the malar bones may precede, by fifteen hours, the initial rigor, and may occur on the opposite side to the subsequent disease. Holding, as he does, that the disease is as strictly uniform in its course, though not in its duration, as variola, he teaches that the mode of treatment should be the same. If the case be normal and regular, no treatment will be required; but certain symptoms may be so serious as to need medical aid. Thus, if the dyspnœa be very great, venesection will give immediate relief, and is advisable in case the patient appears sufficiently strong to support it; it is still more urgently required if the inflamed lung is surrounded by a large extent of congestion or œdema, as evidenced by the presence of sub-crepitant rhonchus; and most of all it is needed when somnolence, torpor, swollen jugulars, and lividity of the face and lips show that the cerebral circulation is seriously impeded. Every one would probably be inclined, as a matter of theory, to agree with the author as to the great, although purely mechanical and temporary, value of blood-letting in such cases; but the practical difficulty is to distinguish between symptoms produced by debility and those which show that the circulation is mechanically obstructed. Jaccoud suggests that, when it is necessary to discover whether feebleness of the pulse is due to the one or to the other of these causes, besides examining the state of the cardiac pulsations, another test may be usefully employed. After examination of the radial artery as usual, press sufficiently firmly upon the vessel to entirely stop the passage of the blood, placing at the same time a finger of the left hand upon the artery just below the point compressed; a pulsation will then be perceived. This is due to the return of the blood through the palmar anastomoses, and if it be perceived immediately after compression, and is as full and regular as the natural pulse, we may be sure that the heart's action is normally vigorous; but if, on the other hand, it is delayed, and, when it is perceived, is notably weaker than the direct pulsation, it is a sign of debility. From some personal observations we have made, we can fully confirm Jaccond's recommendation of this "palmar recurrence" test.

Tartar-emetic and digitalis are, he considers, more frequently indicated than bleeding; they act simply by diminishing the pyrexia, and should only be given when this is considerable; they seldom need be administered for more than two or three days. He gives brandy or wine freely in adynamic cases, but protests energetically against its being employed as the routine treatment;—"it would be as correct to say that every case of pneumonia should be treated by bleeding alone, or by tartar-emetic alone; the same fault is committed, the disease is treated, and not the patient."

Some of the most interesting lectures in the volume are those on *diseases of the nervous system,* as might be expected from the special attention the lecturer is known to have paid to them. We have an excellent description of Progressive Muscular

Atrophy, or Wasting Palsy, dwelling particularly on the way in which the electric current affects the muscles. He states, that those in which the atrophy is but little advanced, and those which will be afterwards attacked, may answer to the electric current as readily as healthy ones, but that their contractile power soon becomes exhausted, and that the current ceases for a time to have any effect; it would also appear that sometimes the loss of contractility under the electric stimulus is the first sign of approaching atrophy in the muscles to which the disease extends; and that the electric current applied to atrophied muscles, although producing no effect upon them, causes violent contraction of their antagonists. These facts are adduced by Jaccoud as clinical arguments in support of the theory which localises the cause of wasting palsy in the nervous, and not in the muscular, system. As such, we confess that we think them wanting in the force of demonstration; which is supplied, however, by the remarkable changes of temperature observed in the subject of this lecture. Not only was the temperature constantly below par on both sides, and lower on the worse side of the body; but there were periods, during which the patient herself complained of cold, and when the thermometer marked a considerable, but unequal, fall. This once amounted to as much as one and a half degrees in the healthier hand, and nine in the other; the paroxysms of chill lasted from two to five hours, and occurred frequently.

Two very interesting lectures are given upon atrophy of the nerve-trunks, on the occasion of a case in which the lumbar and sacral plexuses were compressed, and partly atrophied, by irregularly-distributed fibrous masses on the spinal arachnoid. The symptoms during life resembled those of rapidly-progressing wasting palsy of portions of all four limbs; but were distinguished from it by the loss of power having preceded the atrophy, by the total loss of contractility in all the affected muscles, by the disease following distribution of certain nerves, and by the existence of anæsthesia and analgesia. The diagnosis, atrophy of nerve-trunks, was clearly established during life, although this was not ascribed to compression, but to a spontaneous atrophy, of which Jaccoud relates several instances, where nerves of like origin or function were generally affected. These may throw considerable light on the perplexing cases of local paralysis which Todd, Graves, and others have mentioned.

Another lecture describes a form of sclerosis of the spinal cord which is but little known in England. We are all now tolerably well acquainted with that sclerosis of the posterior columns which is the peculiar anatomical lesion in locomotor ataxia; but Türck, Vulpian and Charcot, and others, have shown (as, indeed, Carswell and Cruveilhier had already partially done) that the same process may take place in detached spots in the cord. It may be interesting, as having a bearing on the pathogeny of an ill-understood disease, to note that this "diffuse sclerosis" (as it is called) resembles locomotor ataxia in its being frequently caused by cold and damp, and in its sometimes (at least) involving the medulla oblongata and the origin of several of the cranial nerves. Jaccoud believes that it may be diagnosed during life by the existence of slowly and irregularly advancing paraplegia, accompanied or preceded by rheumatic or darting pains, of the kind observed in ataxia. A still more unusual form of paraplegia, when sclerosis affects the anterior and lateral columns of the cord, has been described by Jaccoud in his larger work, and, more recently, by M. Bouchard; the symptoms would seem to be those of slowly advancing paraplegia.

As for treatment, Jaccoud considers that the internal and external use of belladonna relieves the violent fits of pain more than anything else; occasionally local applications of chloroform are more successful. He only administers nitrate of silver during the intervals when the patient is comparatively free from pain, having reason to believe that it tends to increase them.

In a lecture on a case of "Chorea gravidarum," Jaccoud gives statistics of thirty-two cases of St. Vitus's dance in pregnant women, collected by himself from different authors, which illustrate some points in the pathology of the disease, and may supplement the account lately given by Dr. John W. Ogle in this 'Review.' In six of these cases only was chorea limited to one side; a rather smaller proportion than in the chorea of children; almost always fright was assigned as a cause. In two-thirds of the cases the disease came on in the course of the first four months of pregnancy; in the same proportion the patients were pregnant for the first time. Four of the cases terminated fatally (by a curious coincidence they are the English ones), eight miscarried before the full term. Jaccoud gave this woman large doses of bromide of potassium; in three weeks she had completely recovered, being then in the fifth month of her pregnancy.

Five lectures are devoted to *albuminuria, Bright's disease, and uræmia;* but these need not delay us long, as the whole subject, and some of our author's views in particular, have been thoroughly examined in a recent number of this 'Review.' To us the account of Bright's disease seems the least happy in the volume, owing to defective arrangement. The disease in general is first described, very much as by Frerichs and the older authors; and it is only in the last lecture that the three varieties established by Virchow are stated. There are, however, some valuable practical hints scattered through the lectures. Thus, special attention is called to the fact, that when a patient suffering from Bright's disease has an attack of pneumonia, the chlorides in the urine are scarcely diminished, and the specific gravity remains low. These characters are so constant, as to lead Jaccoud to suspect Bright's disease in every case of pneumonia in which they are discovered.

He also insists upon looking on cardiac hypertrophy, where it exists, as a certain proof of the long standing and gravity of the renal disease which it complicates. Every practical man will have noticed cases confirming this statement; of course excluding all those where hypertrophy is connected with valvular disease, and, consequently, is not secondary to the renal affection. Jaccoud accepts Traube's mechanical explanation for most of such cases, but considers that the cardiac hypertrophy cannot sometimes be ascribed to disturbance of the circulation by blocking up of renal capillaries, and the great retention of water in the blood, and he thinks that then the altered blood-state, as suggested by Bright, is perhaps the cause.

The clonic variety of convulsions produced by uræmia is well known; our author, however, states that he has seen three cases in the last two years where the blood-poisoning was manifested originally by the tonic spasm of the cervico-dorsal muscles, either alone or associated with tetanus of the other limbs. Such a symptom would be very likely to throw an unwary observer off the true scent, and suggest to him rather cerebro-spinal meningitis, from which the examination of the urine could alone distinguish it.

A still more rare phenomenon of uræmia is related by Jaccoud. He has seen two cases where, with no other uræmic symptoms, the large joints suddenly became swollen and painful; in one case convulsions and coma speedily followed these articular symptoms, in the other, the patient was comatose from the time he was first seen; they both died, and after death the joints were found perfectly healthy, but the kidneys extensively diseased. This combination has not, we believe, been hitherto noted; it is of importance, as such a case would naturally be supposed to be one of acute rheumatism, with secondary cerebral symptoms.

The term "uræmia" to a certain extent prejudges the vexed question of the pathogeny of the cerebro-spinal symptoms produced by renal disease; and in effect Frerich's theory of blood-poisoning by carbonate of ammonia is the one generally held at the present day.[1] We think, with our author, that this is being too exclusive; that there are, on the one hand, cases where the autopsy proves the existence of œdema or (more rarely) dropsy of the brain, and, on the other, where the excretion of the urea was at or above par at the time that the cerebral symptoms occurred. Jaccoud therefore holds that we must admit three different causes for these symptoms. In patients who are already dropsical, and in whom the urine is decidedly albuminous, but with no great deficiency of urea, they are probably due to œdema and consequent anæmia of the brain, as suggested by Traube; vomiting, diarrhœa, and convulsions are rare, the most prominent phenomena being somnolence ending in coma. In the cases where poisoning by carbonate of ammonia is the probable cause (Jaccoud suggests the word "ammoniæmia") the excretion of urea is greatly diminished, the mouth is dry, there is considerable thirst, vomiting and diarrhœa are generally observed, and free ammonia is detected in the expired air, urine, vomit, or fæces; in such cases the convulsive symptoms are chiefly noticed. In the third class of cases, where there is no great amount of dropsy, and no evidence of diminished excretion of urea, Schottin and other chemists have shown that some of the extractive matters, normally got rid of by the urine, are retained in the body, and they suppose them to be the noxious agents.

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[1] Neitz and Jacksch of Prague teach that the urea is decomposed into carbonate of ammonia, not in the blood, bnt in the intestines, whence it is absorbed.

Rigler has recently pointed out that the symptoms produced by re-absorption of urine, retained owing to vesical, prostatic, or urethral disease, are essentially those of "ammoniæmia;" there is almost invariably vomiting or diarrhœa, the tongue is coated; the urine and the expired air contain ammonia, and eclampsia often precedes the final coma.

In his very careful account of *saccharine diabetes*, the author does not succeed in throwing any new light on the pathology of this disease; but he calls special attention to the most recent analyses having shown that the urine is affected in other ways besides the presence of sugar. Thus Maly has lately proved that the quantity of creatinine may be very enormously increased, and Böcker and Moslen have shown that the amount of sulphates is frequently more than doubled, while that of the chlorides may be even more than tripled; the phosphates generally remain unaltered. More important, in a chemical point of view, is the increase of urea which is often observed; Jaccoud urges that this azoturia is the ordinary cause of the rapid wasting observed in some patients, who excrete no more sugar than others in a better general state.

The volume ends with a lecture on the qualitative examination of the urine, where we notice what is, we believe, a new test for bile in that secretion, which the author tells us is much more delicate than those commonly employed. The urine is placed in a test-glass, and a glass rod, which has been first dipped into the strongest monohydrated nitric acid, is plunged into the upper part of the liquid; the acid falls to the bottom and takes with it a certain amount of grass-green colouring matter, derived from the bile: if the quantity of biliary colouring matter be very small it may be necessary to repeat this three or four times.

We do not propose to notice here the lectures on cardiac and hepatic diseases, because, although they are very clear and instructive, they do not present any points of sufficient novelty to justify our prolonging this review; but we will end by extracting a few miscellaneous items which have a practical interest.

For cardiac hypertrophy, digitalis is, of course, stated to be the classical remedy; but Jaccoud confirms Dr. C. J. Williams' opinion of the good effects of iodide of potassium, which he gives in rather large doses; and adds that caffein, in doses of from three to fifteen grains a day, acts quite as rapidly and certainly as digitalis, but does not produce so great or permanent an effect. It may be worth considering whether the hypodermic injection of this substance would not increase its powers. He treats albuminuria of cardiac origin on Vogel's plan, with perchloride of iron and tannin, as soon as the dropsy has been removed by purgatives; while in albuminuria owing to syphilis he has invariably found that the administration of iodide of potassium and corrosive sublimate together effect a cure, when each separately had failed to do any good. This appears to be his usual mode of treating all the visceral results of syphilis; he gives the iodide in doses which most English practitioners would think dangerous, beginning with fifteen grains, and increasing rapidly up to one drachm, or one drachm and a quarter, daily.

The account we have given of this volume must not be taken to have by any means exhausted it; our readers, to whom we earnestly commend it, will find in every lecture many facts and useful suggestions which our space has obliged us to pass unnoticed. We trust that Professor Jaccoud may soon add, to his own reputation and our great advantage, another series of equally valuable lectures, to the ones we have now before us.

REVIEW V.

1. *Traité Experimental et Clinique de la Regeneration des Os et de la Production Artificielle du Tissu Osseux.* Par L. OLLIER, Chirurgien en Chef de l'Hôtel-Dieu de Lyon. Paris. Victor Masson et Fils. 1867.

Experimental and Clinical Treatise on the Regeneration of Bones and Artificial Production of Bony Tissue. By L. OLLIER, Surgeon-in-Chief of l'Hôtel-Dieu de Lyon. Paris. Victor Masson et Fils. 1867.

2. *De l'Evidement Sous-périosté des Os.* Par M. Le Docteur CH. SEDILLOT, Prof. de Clinique Chirurgicale à la Faculté de Médecine de Strasbourg, &c. 2nd ed. Paris. J. B. Baillière et Fils. 1867.

Concerning Sub-periosteal Gouging of Bones. By M. le Docteur CH. SEDILLOT, Prof. of Clinical Surgery to the Faculty of Medicine of Strasbourg. 2nd ed. Paris. J. B. Ballière et Fils. 1867.

3. *Periosteal Preservation in Operative Surgery.* Paper presented to the Surgical Section of the Brit. Med. Assoc. in Oxford, August, 1868. By WILLIAM STOKES, jun., M.D., Surgeon to the Richmond Surgical Hospital, and Lecturer on Surgery in the Carmichael School of Medicine.

M. Ollier's work is divided into two parts; the first experimental, the second clinical. Ten years ago, we are told in the preface, he showed the great importance of preserving to the utmost, in articular resections, the continuity of the sheath formed by the periosteum and capsular ligament around the bones of the joint, and especially because by so doing a true reconstruction of the joint could occur—of a joint, that is to say, not merely in the sense of two bones which move one on the other, but of an articulation formed after a similar pattern to that which had been removed. At about the same period, he had also demonstrated the ossification of the periosteum when detached from its natural connections, and the slight risk of necrosis of the bones from which it had been removed.

M. Ollier devotes a long chapter in his first volume to the history, from Hippocrates downwards, of the various theories held by eminent physiologists and surgeons concerning the regeneration of bone; but, instead of quoting any of these, it will be more profitable, in our limited space, to pass at once to the consideration of the experiments of the author himself, many of which have been already published, and are now well known.

The first point to be decided was the true function of the periosteum, whether it was or was not to be considered the principal agent in the regeneration of bone; and to determine this matter, M. Ollier made a large number of experiments, chiefly on young rabbits. In the first series of experiments, a flap of periosteum was dissected from the surface of a bone, but not completely detached. The free end of the flap was then passed around the muscles in the neighbourhood and brought close up to the end which remained attached to the bone. It was fixed by suture to the skin to prevent retraction. The outer wound was then closed. Of the first experiment described, which was performed on the tibia of a young rabbit, the following was the result:—Some little swelling of the limb succeeded the operation; within a week the periosteal flap could be felt increased in size and consistence, and within a fortnight already hardened. In six weeks the animal was killed, and, on examination, it was found that the periosteal fibrous ring had become a bony one, fixed to the tibia by one end and free at the other. The new bone looked normal; it was surrounded by periosteum, and had a smooth and compact surface. On section, the interior looked as if it were becoming hollowed out. Under the microscope the ordinary characters of bone-tissue were discovered.

In other experiments on equally young rabbits similar results were obtained. When the flap of periosteum was not rolled around the neighbouring muscles, or when the suture at its free extremity cut through the skin too quickly, the flap shortened itself somewhat, and the ossification produced, as might be expected, a more or less irregular tubercle instead of a flattened plate or ring.

Experiments on older animals were much less successful. In two such experiments, practised at an interval of ten days, on an old-looking doe rabbit about five years of age, suppuration occurred, and was followed by very slight ossification at the base of the periosteal flap in one wound and by no ossification at all in the other.

Since the publication of these earlier trials, M. Ollier has repeated them on other animals, especially the dog and cat, with success. In the dog, however, there was more difficulty in obtaining ossification, chiefly on account of suppuration in the neighbourhood of the transplanted periosteal flap.

In a second series of experiments a similar operation to that already described was performed; but three days afterwards the wound was re-opened, and a small piece was cut out from that end of the periosteal flap which had been left attached to the bone. In an experiment of this kind which is given by the author in detail, the flap of periosteum was found, on opening the wound, already swollen and hardened. A month afterwards the animal was killed, and the flap was found perfectly ossified, and forming a little bony bridge over the muscle around which it had been wrapped. At two points also it had become re-united to the bone.

In a third series of experiments the periosteal flap was not only dissected up from the bone as before, but completely detached from it, and left adherent to other parts only by a few muscular fibres. Even this last connection was given up in later experiments; and the periosteum, dissected completely from bone, tendon, and muscle, was transplanted to another part of the body altogether, as, for instance, from the tibia to the subcutaneous tissue of the forehead. This plan also was successful; and the operator had the satisfaction of finding, after a time, the transplanted piece of periosteum ossified, as in the experiments in which some kind of connection between it and the neighbouring parts was preserved. The little bit of bone thus produced was surrounded by periosteum and contained marrow.

To succeed, M. Ollier acknowledges that these transplantation experiments must be performed on animals which are young and growing. As regards the subjects best suited for them, he remarks that in ruminants they are less successful than in carnivora, from the wounds suppurating more commonly and more freely. In old subjects such operations are unsuccessful. The transplanted flap may possibly adhere and become vascular, but it remains fibrous, and finally disappears altogether by absorption.

The periosteum of all bones and of all parts of a bone does not ossify with equal readiness. That of the long bones is better for this purpose than that of the flat; and of the former, that belonging to the shaft near its junction with the epiphysis is better than that upon any other portion of the bone. It is least good for transplantation where the muscular fibres are inserted upon it, being here much thinner than elsewhere.

Among the most interesting of the author's experiments was one on the dura mater. A small portion of this membrane from the vertex and another from the base of the skull were transplanted, immediately after death, from a young rabbit six weeks old to another rabbit of two months—one piece to the subcutaneous tissue of the right groin, and the other to that of the left. A minute portion of bone was formed in both regions—in one about the size of a grain of corn, and in the other about half as much; in the latter instance not in a single mass but in the form of scattered granules. In adult animals experimented on no ossification took place.

With the exception of the dura mater (if, indeed, it can be called an exception) no other fibrous membrane than the periosteum was found on transplantation to produce bone. If the transplanted membrane lived and contracted any adhesion at all, it remained fibrous only, and was in most instances soon absorbed without leaving any trace of its presence.

M. Ollier's experiments on the production of bone were not confined to the periosteum. Having proved the "osteogenetic" properties of this membrane, he next attacked the medulla. His investigations, however, in this direction produced only negative or doubtful results. The experiments in which the medulla was transplanted failed altogether so far as the formation of bone was concerned. In a trial, however, of a different kind he was more successful. Having amputated the fore leg of a young rabbit three months old, he introduced into the exposed and cut end of the medullary canal both of the radius and ulna a thin silver tube, so as to isolate the marrow from the bone around. Some bony matter was formed within the tube inserted into the radius, but not in the ulna.

For practical purposes it was evidently necessary to find out whether, after these periosteal operations, there was any or much danger of causing necrosis of the bone from which the investing membrane was thus so unceremoniously removed; and M. Ollier experimented directly on this point. He came to the conclusion, however, after many trials, that there was much less danger of this evil result, especially in young animals, than might be expected.

Another interesting question also arose, whether the new periosteum, which re-clothes the bone, has the same bone-forming power as the old. In an experiment undertaken for the purpose of testing this matter, the periosteum was dissected off the front of a young rabbit's tibia. It was replaced by a new membrane in about seven weeks; and at the end of this time the new layer of periosteum was in part dissected off the bone, as in the experiments first related, and rolled around the neighbouring muscles. Ossification was found to occur in the flap, but to a decidedly less extent than in the experiments with natural periosteum. Very imperfect traces of ossification were, moreever, found to follow transplantation of "secondary" periosteum to a distance. After a certain number of renewals the power of forming bone ceases altogether; so that M. Ollier failed to get any ossification in periosteum of the third generation.

In some experiments with perichondrium resembling those with periosteum the author found that no fresh cartilage was produced. Some little bony granules were found, however, in the perichondrium of the epiphysial cartilage—that of permanent cartilage remaining fibrous.

The author found that cartilage deprived of its investing membrane cannot be grafted on another part of the body. Ossification may, however, occur in temporary cartilage when its perichondrium is left intact, as was shown in the following experiment. A small round piece of epiphysial cartilage surrounded by the perichondrium was removed from the lower end of the radius of a young rabbit about six weeks old, and planted under the skin of the forehead of another rabbit. The operation was performed on the 28th of March, and on the 17th of June the small graft of cartilage was found adherent to the surrounding cellular tissue, and with the appearance and consistence of bone. The microscopic characters are not mentioned.

Having proved the importance of the periosteum in the process of ossification by the foregoing experiments, M. Ollier tested its utmost power by removing whole bones, first carefully dissecting off their periosteum, and, as far as possible, leaving the connections of the latter with surrounding parts uninjured. After this fashion the radius and other bones were operated on with success, tolerably complete regeneration taking place in many cases. No reproduction occurred when the periosteum was removed with the bone.

After the consideration of subperiosteal resections of bones in the manner already indicated, the author proceeds to describe his experiments on the excision of joints, and dilates at some length on the advantages of preserving, as far as possible, in these operations the integrity of the periosteum and ligaments which surround the articular ends of the bones. Cases of resection of the joints of young animals are then given. The results were extremely good, the general shape and appearance of the bones being well reproduced. As an instance we may quote the case of a young dog, five or six months old, from which the elbow-joint was completely removed by the subperiosteal method, together with a third of the shaft of the humerus and a considerable portion of the upper ends of the radius and ulna. The dog was killed two and a half months after the operation. On examining the joint it was found that a well-marked trochlea had been reproduced. There were also two well-marked and partly ossified condyles with a distinct and deep intercondyloid fossa. The bones of the forearm had been somewhat drawn back by the triceps. The new olecranon was more curved than the old one, and the ossification had extended a little into the tendon of the triceps. The extremity of the radius was surrounded by fibrous tissue incorporated with the capsule; its articular cup was not reproduced. The articular surfaces of the bones were joined together by a thick capsule in which were found strong lateral ligaments. The muscles had preserved their normal attachments. The movements of flexion and extension were re-established, the latter rather limited by the too great curvature of the new olecranon. The paw was flexed on the fore-arm by the retraction of the flexor muscles.

An instance of excision of the knee-joint, also in a dog, is related; and if the amount of bone removed be taken into consideration the result must be considered very good. About the same results were obtained after excisions of the articular and arthrodial joints. All these experiments were performed on young animals. In older subjects less successful results were obtained; but even in these the sub-periosteal method was proved to be incontestably superior. In all cases, old and young, removal of the periosteum with the bone was followed by little or no regeneration.

In some experiments on the growth of bones, M. Ollier proved that increase in length takes place in the arm bones chiefly at the upper extremity of the humerus and the lower ends of the radius and ulna; while the opposite occurs in the leg. This, as the author observes, is not a mere re-statement of the fact that the epiphyses of the bones of the arm and fore-arm are joined to the diaphyses sooner at the ends of the latter nearest the elbow than at the other, while the reverse holds good for the bones of the leg. For the increased growth takes place before either of the epiphyses has become united to the diaphysis; and, moreover, the times of normal junction of these are not sufficiently distant, one from the other, to account for more than a very small increase at one extremity above that which occurs at the other. The two facts, however, go together. Where the union of epiphysis with shaft is quickest, there is also the least contribution to the growth of the bone. In man, also, as Berard has noticed, there is another fact to be placed with these two,—that the nutrient canal is directed towards the end of the bone at which are the quickest completion of ossification and the least growth. Although, however, this last fact is true in man, its connection with the other two points to which reference has been made is a mere accident, and in no sense physiologically related to them. In the rabbit the nutrient canal of both femur and humerus are directed downwards, but the law of ossification and growth in these bones is the same as in man.

M. Ollier was unsuccessful in all his endeavours to trans-plant the periosteum of one species of animal to another, except in one rather doubtful instance in which a piece of periosteum from a dog was planted under the skin of a rabbit. There was no special difficulty in grafting from one variety of rabbit to another.

He found that, if kept at a low temperature, the periosteum of an animal dead some hours could be successfully grafted on the living.

The last part of the first volume is occupied with the consideration of graftings of bone itself. In one of the experiments which may be quoted as a typically successful one, a piece of the whole circumference of the right radius of a young rabbit, eight months old, was fitted into an interval in the left radius made by taking away from it a similar piece; and *vice versâ*. On one side the experiment succeeded well, and perfect bony union was the result. On the other suppuration occurred, and only the periosteum of the graft lived.

As may be gathered from his experiments on the lower animals, the description of which occupies the greater part of the first volume, M. Ollier is a strenuous advocate for sub-periosteal resections, not only as against resections performed in the ordinary manner, but in contra-distinction to another operation of the kind, '*évidement*,' the supporters of which, headed by M. Sedillot, claim for it all the advantages of M. Ollier's operation with freedom from many of its perils.

The operation of *évidement* is no new one. There is scarcely any more ancient. Essentially it consists in scooping away all the diseased parts of a bone so as to leave a cavity with healthy bony walls, and has been practised by surgeons from Celsus downwards. M. Sedillot has, however, employed the method in many cases in which the majority of surgeons would have resected or amputated the diseased bone. His object has been the same as M. Ollier's, namely, to make use of the periosteum as a means of regenerating lost bone. But while M. Ollier dissects off the periosteum by itself as completely as possible from the underlying bone, M. Sedillot scoops away the bone from within, and leaves of it only a thin shell to support the periosteum, to which, and not to the shell of bone, he gives the credit of the regeneration which takes place. He claims, therefore, for this operation the same purpose or intention as that of M. Olliers, with a better means of carrying it into effect. M. Ollier denies both statements. He believes that the regeneration which follows *évidement* is due to the bony shell which is left; and that the latter is not, as his opponent in argument believes, a mere temporary mould destined to disappear when the periosteum has reproduced sufficient new bone to enable it to dispense with its services. He denies, too, emphatically, that *évidement* is for all or even the majority of cases so good an operation as sub-periosteal resection; and although he admits, with all other surgeons, that excellent results are often obtained by its means, especially in chronic inflammatory conditions of the shafts of bones, or of their epiphyses when there is no communication with the neighbouring joint, he is a very decided opponent of the operation of scooping out the articular extremities of the bones of a large joint.

Having thus disposed of the rival operation of *évidement*, M. Ollier proceeds to relate his clinical experience of the value of sub-periosteal resections; and to silence all doubts respecting the true regeneration of bone after operations of this kind, he relates a case of excision of the upper end of the humerus. The case has been previously published, but it will be well to trace briefly its main outlines. It was that of a girl, fifteen years old, from whom M. Ollier removed the upper half of the humerus on account of chronic inflammatory disease, not, he specially adds, necrosis. The periosteum was carefully preserved in the operation and dissected, where still adherent, from the subjacent bone. The ligaments and tendons were also dissected from the head of the bone without cutting them, and the upper part of the bone being thus stripped completely naked was sawn off. In this way a periosteal and ligamentous sheath was left tolerably complete. The patient had a long and chequered convalescence, but at last recovered well with reproduction of bone within and from the periosteal sheath. The proof of this last statement, namely, that the regenerated bone was produced from the periosteum and not from the cut extremity of the old bone, lay chiefly in the fact that during convalescence there was for some time mobility of the upper and new piece of the humerus on the lower and old part, which could not have happened had the new bone been developed continuously with the old. The movements of the shoulder-joint were fairly restored, and although no new head to the humerus could ever be felt, there was evidently the restoration of a scapulo-humeral joint of the same type as the original. For, adds M. Ollier, not only were the chief movements restored, but the humerus could be rotated on its own axis, and the deltoid separated the elbow from the trunk to a distance of six inches. To objectors to sub-periosteal resections on the score of the difficulty of separating the periosteum from the bone the author points out that the membrane is thicker and less adherent in young subjects than old, and thicker and less adherent in inflamed than in healthy bone.

The next portion of M. Ollier's work is devoted to the consideration of the best method of performing the operation, and he lays especial stress on the advisability of cutting neither muscle nor tendon; and if the former be divided of necessity, its nerve should be carefully avoided. By thus operating carefully, the tendons are left with their attachments to the periosteum uninjured; and, in resections of joints, there remains an almost uninterrupted fibrous *loge* formed by periosteum, tendons, ligaments, and capsule, to which the muscles preserve their proper attachments. Other practical instructions are given by the author, and will well repay perusal. The following are his general rules for the operation as performed on the joints:

1. A single straight or sinuous incision through the skin in the direction of the long axis of the limb. If absolutely necessary, incisions perpendicular to the main one must be made.
2. A single straight incision of the capsule.
3. The ends of the bones should be dislocated before being sawn in almost all cases but those of the wrist and ankle.
4. If the condition of the bones beyond the line of section require it, the gouge may be used; but *évidement* should not be practised before resection.
5. The resected bones should be kept extended unless when a bony anchylosis is required. The degree of extension must be in proportion to the expectation of regeneration.

Special instructions are also given by the author for the resection of each joint.

M. Ollier's favourite instrument is one which he has named the "Sonde-rugine," and which is a long gouge, curved so as to allow of its being used as a protection to the periosteum after it has separated it from the bone. It is pierced at its extremity, so as to serve for carrying a chain-saw around the bone after the separation of the periosteum. For cutting the bone he prefers the bone forceps when practicable and the chain-saw when a saw is absolutely necessary; the great object being to divide the bone with as little difficulty as possible, and to avoid all disturbance, especially of the periosteum, beyond the line at which the section is to be made.

With the operation of sub-periosteal resection the use of nitrate of silver or the actual cautery may be combined, when fungous granulations or fistulous tracks seem to render either of them necessary.

The degree of extension to be kept up after the operation depends on the chance of a good result. In a young subject, for instance, with a tolerably healthy state of the bone, much more regeneration may, of course, be expected than in the presence of the opposite conditions, and therefore more extension may be safely applied.

Several cases in which sub-periosteal resections have been performed are related by the author; but, so far as regards the regeneration of bone, much less brilliant results have been obtained for obvious reasons than those which followed experimental resections in the lower animals. The cases, however, prove very fairly the advantage of preserving the periosteum as much as possible. Reference may be made especially to operations on the jaws, clavicle, humerus, and ulna—the last in a woman fifty-four years old. One or two cases of the same operation on the tibia were followed also by excellent results.

With resections of joints also M. Ollier has had good success. Reference may be made particularly to the tabular account of operations on the elbow-joint.

The last part of the work is chiefly occupied with a notice of what have been termed "osteoplastic resections," and by the author "preliminary osteotomy;" in other words section with temporary displacement of bones for the more thorough exposure and removal of tumours concealed by them. M. Ollier gives a detailed account of what he believes to be the best operation of the kind for the removal of naso-pharyngeal polypi. A horseshoe incision is made over the bridge of the nose, the highest part of the curve falling over the naso-frontal depression, and the two ends descending to the posterior borders of the alæ nasi. One cut across from right to left should extend down to the bone. The knife is then laid aside, and a narrow-bladed saw made to cut rapidly downwards in the same direction as the external wound, stopping when the nasal processes of the upper maxillæ have been divided. With scissors the septum or lateral cartilages are then sufficiently divided, and the nose is turned down. The anterior opening of the nasal fossæ thus exposed may not offer space enough through which to attack the polypus; and in this case it will be necessary to displace the septum to one or other side. This is readily enough done in most cases by the forcible introduction of the finger; but if necessary the displacement may be made easier by a snip with the scissors. Very commonly the polypus has itself partially done the work by pushing the septum to one side. After exploration with the finger of its connections and "surroundings," the polypus is now seized with strong forceps, and, after extraction, the part to which it was attached—most often the basilar process of the occipital bone—may be gouged, the gouge being guided by a finger in the mouth.

By this method of operating M. Ollier has extracted successfully, from a youth of nineteen, a naso-pharyngeal polypus springing from the basilar process of the occipital bone and body of the sphenoid and weighing rather more than seven ounces. Of seven patients thus operated on only one died from the effects of the operation; and in this instance the root of the tumour had perforated the base of the skull and extended into the middle lobe of the brain. In no instance was there the least cause for anxiety after the operation, concerning the vitality of the nose.

The author prefers the section of the nose from above, as just described, to the operation from below which was performed by the late Sir William Lawrence a few years since for the extraction of a mass of mucous polypi filling the nasal fossæ.

After reading the good results of subperiosteal resections related by M. Ollier, we confess to a slight shock on taking up the book of M. Sedillot. He says, subperiosteal resections should be rejected as illogical, inefficacious, and dangerous:—"We maintain that the periosteum, completely separated from the bone beneath, whether it be left in its place or taken to another part of the body, as a sheath or flap, will be of no service to surgery as the means and organ of regeneration of bones." And many other sentences of the same kind might be quoted. Referring to M. Ollier's experiments on the lower animals, he remarks that curious as they are physiologically, they are useless to surgery.

We have already referred to M. Sedillot's operations of *évidement*, and noticed his theory on the matter, with its difference from M. Ollier's. The present work seems intended to show the superiority of this operation to subperiosteal resections. Theoretically, M. Sedillot's position is a good one. "The first indication is to avoid the sacrifice of any healthy part of the skeleton, since this portion, clothed by its periosteum, is the most efficaceous and certain means of reproducing the whole of the bone. Such is the foundation of our method of *évidement*, and our gravest objection to subperiosteal resection." Like his opponent, he refers to the benefit derivable from leaving unharmed the connections of the periosteum with the parts around. So far, of course, the author is justified in holding his own; but, in the face of M. Ollier's cases, as well as those of others, the following sentence is rather inexplicable:—"What has happened to the partisans of subperiosteal resections? They have taken away the bone. The periosteum, unsupported, becomes shrunken, and is folded by the contraction of the muscles; it inflames and suppurates. In the majority of cases the limb is shortened and deformed, with no production of new bone."

Some experiments on young dogs, to test the degree of repair possible after gouging out the interior of bones, and leaving only a thin layer in contact with the periosteum, were undertaken by M. Sedillot, and followed by good results:—"Les évidements des diaphyses et des extrémités des os sans destruction des lames osseuses periphériques et par consequent sans penetration dans les jointures, ont toujours réussi."

The cases of disease in which *évidement* is useful and practicable, according to M. Sedillot, are, chronic inflammations of bone, tubercular disease of the articular extremities, fibrous, vascular, encysted, and certain limited enchondromatous tumours. The operation, too, may be employed as an assistant to articular resections, and by its means much less bone is required to be removed. The operation itself, as we have said before, resembles very nearly that undertaken so frequently by almost all surgeons for the removal of carious bone, but involves more methodical dissection of the periosteum than is common in this country; and the gouge is especially employed so as to leave only a thin shell of bone immediately within and supporting the periosteum.

A large part of M. Sédillot's work is occupied by the relation of instances in which the foregoing operation has been employed by himself or others. The difference, however, between his method and that of other surgeons is not so great that we can afford space for quoting from his cases. It seems to us rather that these operations should be classed with the ordinary gouge operations, and the two compared with subperiosteal resections as performed by M. Ollier and others.

Dr. Stokes's paper on "Periosteal Preservation in Operative Surgery" is probably fresh in the memory of most of our readers. It is a very good and practical *résumé* of his own personal experience on the subject. He is an earnest supporter of Professors Syme and Langenbeck and M. Ollier; and, indeed, his attention was specially directed to this subject during attendance on the cliniques of the two last-named surgeons.

The two following cases may be quoted from Dr. Stokes's experience in this matter; and he relates several others.—

> "The first of these operations was performed on a girl aged 12, of delicate habit, who was under my care in the autumn of 1864, and who suffered from strumous disease in her left elbow-joint. The patient stated that, four months previous to her admission into hospital, she received a fall in which the elbow was struck against the ground with great violence. After falling, the patient suffered severe pain in the joint; it swelled greatly; and she found it impossible to extend or 'stretch' it, to use the patient's own expression. The inability of extension of the arm remained a permanent condition. Three months after the accident, the patient was admitted into my wards in the hospital. I found the arm forcibly

flexed; extension was impossible. The soft parts about the joint were swollen and puffy, so that it was quite impossible to determine whether the original injury was an epiphysary disjunction, which the age of the patient rendered probable, or a simple luxation. There were three or four sinuses, situated over and in the immediate vicinity of the internal condyle; and, on introducing a probe into any one of these, the existence of soft diseased bone could be readily determined. I accordingly performed the operation, adopting the long vertical incision, carrying it over the ulnar side of the olecranon, and, taking especial care not to divide the triceps tendon, proceeded to raise the periosteal envelope by means of the various formed raspatories devised for the purpose. The elevation of the periosteum was not attended with any great difficulty, and the remaining steps of the operation were then completed in the ordinary manner. The limb was afterwards put up in a gypsum bandage, and kept in a continual bath for four weeks. After this, the wound being all but healed, gradually flexion and extension were commenced. Three months and a half after the operation, the power and flexion and extension of the joint was almost perfect; and now, at a period of little more than three years after the operation, during which period there has not been the slightest evidence of a re-appearance of the disease, the patient enjoys every motion that the healthy joint possesses, perfect power of flexion, extension, pronation, and supination; and, what bears more directly on the immediate subject of this communication, there is a most obvious osseous re-formation, not only of the condyles, but also of the olecranon. This re-formation of the olecranon has also been frequently observed by Langenbeck, and also by Huctor, one of his former assistants."

The other case which we shall quote from Dr. Stokes's paper also, as he remarks, affords a striking illustration of the osteogenetic properties of periosteum, and, consequently, of the great practical importance of preserving it in resections.

"The patient, a young woman, about 19 years of age, suffered from suppurative osteitis of the right ulna, the result of a severe blow. A free incision was made down to the bones; and over this, which I found extensively diseased, the periosteum was much thickened. Having divided the membrane by one of Nélaton's so-called periosteal knives, I then detached it by means of raspatories from off each side of the bone. The diseased portion of bone, about two and half inches in length, was then removed by small resection-saws. Three months after the operation, the wound was completely healed. New bone had formed throughout, and the patient was enabled to return to her employment, which was that of a house-servant."

Dr. Stokes has also practised with success M. Ollier's operation for the restoration of the bone by including in his flap periosteum from the frontal bone. The case was one of a young man whose nasal bones had been completely destroyed by lupus. "About fifteen months after the operation, I inserted in two different situations in the nose the so-called 'entomologist' pins down to the newly formed bone, and satisfied myself and others present, beyond all doubt, as to its existence."

It is impossible to give an opinion worth very much concerning the value of an operation the details of which are known only from the descriptions of others. It is, indeed, easy enough sometimes to agree with an operator in his estimate of his own operation; but, much more commonly, it is far easier to disagree with him and pick holes in both his theory and practice. Nothing is easier than to find objections to M. Ollier's views on the reproduction of bone; while his most successful experiments on the transplantation of periosteum in the lower animals may be met by the objection that operations on healthy young dogs and rabbits are performed under conditions utterly unlike those which could be met with in man. And so they are. There is no doubt of it at all. And if these experiments are to be used as guides to what may be expected from dealing in a similar fashion with the bones of scrofulous children, it might be better for them not to have been performed at all. Taken so, they will only lead to mischievous practice, unfounded expectations, and disappointment. Taken for what they are worth, as illustrations of what the periosteum will do under the conditions of youth and health, they are most valuable contributions to our knowledge of the properties of this membrane, and should prove very useful to surgery. But, after all, the value of sub-periosteal resections must be settled alone by clinical practice and long comparison, side by side, with the results of other methods of resecting. So many practical circumstances must be taken into account, that the matter cannot be decided in any other way. The cases, however good, obtained by one or two operators, will not settle the absolute value of any operation. That a surgeon can cure a patient in one way better than another is not a conclusive proof that the same would happen to everybody else. In the case of a new operation, especially, or one in which the operator takes some particular pride, if one may be allowed to use the expression, how much of the success may depend on attention to small matters which will be probably disregarded by those having no unusual interest or stake in the result. The details may be so small that the operator is himself unconscious of heeding them. But he employs them nevertheless; and their employment may be the secret of his success. That the best mode of operating may depend as much on the surgeon as the patient is not always sufficiently considered. A man of impatient disposition, or of a mind little given to consider petty things, would do well, probably, never to treat a popliteal aneurism by any other means than a ligature on the femoral artery; and there can be little doubt that a careless and blundering operator or an invariably quick one, ought not to perform a sub-periosteal resection.

It is from this point of view, although by no means from this alone, that sub-periosteal resections must be looked at. We feel sure that there are many surgeons who could never be successful with them, and that they will never replace altogether excisions performed in the ordinary manner. But, at the same time, the results obtained by those who practise them habitually are far too good to deserve the hasty if not careless investigation hitherto accorded to them. Rejection should at least follow examination, and not go before it.

REVIEW VI.

Annali Universali di Medicina. Compilati del Dottore ROMOLO GRIFFINI. *Milano,* nella Galleria de Christoforis.

Universal Annals of Medicine. Compiled by Doctor ROMULUS GRIFFINI. *Milan,* in the Christoforis Gallery.

WHILE Italy has recently accomplished considerable improvements in her social, literary, and general condition, medical science and education, as regards professional men, have made decided progress throughout those districts of the Peninsula which are now governed by the existing representative legislature. Such being the fact, and believing it would prove interesting to British readers, we propose, in the subsequent pages, to give a brief sketch of several prominent Italian universities possessing medical schools, as also short notices of a few charitable institutions for treating disease. For these purposes, the title of one of the best medical Italian periodicals has been placed at the head of this article, because it forms an appropriate medium for communicating to British physicians and surgeons recent and, we hope, interesting knowledge respecting their Italian *confrères* and national establishments.

Although Italy possesses at the present moment sixteen universities, exclusive of the "Sapienza" at Rome, which likewise should be added to that category, several scarcely deserve to be considered as important medical schools. Nevertheless, some of them very well deserve that appellation, as, for instance, Pavia, Bologna, and Padua, in northern Italy; while Naples, on account of its numerous professors, valuable educational appliances, but especially on account of the immense assemblage of students there belonging to its several faculties, particularly that of medicine, far outstrips every other analogous institution, not only in Italy, but generally throughout Europe. This great superiority in point of numbers is shown by the fact, that the aggregate students affiliated to the University of Naples exceeded 11,500 during the academical session 1866-67, and whereof above 4,000 were medical. Indeed, at some of the lectures, often 500 auditors were recently attending a favourite teacher. This university dates its foundation from the thirteenth century, has fifty-four professors, and contains the usual number of faculties.

Notwithstanding its often crowded class-rooms, and being located in a populous city, recently metropolitan, besides having collateral advantages with numerous public institutions, which always prove highly valuable towards obtaining practical knowledge, the Neapolitan medical school does not take such a high scientific position as the Alma maters named previously, even although these three are located, particularly Pavia and Padua, in small provincial towns. Nevertheless, this university possesses important advantages, particularly in regard to the extensive means which Naples affords towards obtaining medical and surgical experience, at its large public institutions for treating patients whilst labouring under disease, or suffering from accidents.

Among the Neapolitan hospitals where pupils attend, "La Santa Casa degli Incurabili" contains the largest number of inmates, having on an average about 2000 in-patients, both sexes included. Here every form of malady comes under observation. Many doubtless are chronic cases; still the establishment supplies an ample field for a diligent student's observation. Among the inmates seen lately in this charity by an English physician when visiting Naples, it may not be here altogether uninteresting to medical readers, if mention is made of a resident female, who, by the institution records, had then attained her 113th year, and was at that period in fair bodily health. She was born in Madrid during 1755, and named "Maria Giovanna Castalda," her father being Spanish, but mother Neapolitan. When three months old the infant came to Naples with her parents, in the suite of Charles III, who succeeded an elder brother as sovereign of the Two Sicilies, on his ascending the Spanish throne. Besides other points ascertained, it may be stated, this venerable relic of antiquity had fractured her thigh bone two years previously, and although it united, she had since continued bedridden; while further she stated, her father's father had died 118 years old, and two uncles on the same side, at 116 and 121 respectively. Notwithstanding no birth registers could be referred to in proof of such statements, still, the "Segnora" being reported on the hospital books as of the great age mentioned, warrants the present notice; and if it be really true, chroniclers of curious facts may say, "This old lady, although born even before the younger Bourbon dynasty began to reign at Naples, actually lived to witness their exile from that kingdom, after the lapse of more than a century."

The "Ospedale de Pelligrini" in the city suburbs, also receives sick inmates, particularly convalescents, and there is likewise the one named "Della Pace," where patients labouring under acute disease are admitted. It is capable of receiving 200 inmates, and may well be pointed out as the cleanest, best ventilated, and altogether the most superior receptacle for diseased persons at present existing throughout Italy. The surgical ward may be especially commended for great cleanliness, excellent ventilation, and general arrangements. It is high roofed, and has large windows on each side, the only fault being its extent, which objection, however, could be easily remedied, if divided into two separate apartments.

A clinical hospital has also been recently constructed, or rather, an old convent for religious sisters was altered for that object. The wards are generally small, well ventilated by cross windows, and are lofty. There is an ample supply of water; while altogether, the institution is well arranged, and contains about 400 patients. It has four different amphitheatres, with every appliance for clinical instruction by respective professors. Besides which, very recently a department for treating the diseases of infants has been added, into which the sick patients are admitted, in order that lectures may be given by professors.

The syphilitic or Lock Hospital likewise affords an excellent school for studying the various forms of that class of maladies. It contains usually about 300 inmates, most being public prostitutes sent there by the police. The building is old, and not well arranged. However, as important alterations are in progress, or proposed, this institution will become improved, and hence more salubrious. Being the establishment to which all diseased prostitutes belonging to Naples are sent for treatment, it appears relevant to the subject to give a few statististical facts regarding that class of unfortunates, who are placed under more strict police regulations than in almost any other European municipality.

The number of public women licensed to follow their vocation was 1509 during a recent year, of which an official report has been published. Of these, 753 were natives of Naples or its province, 685 belonged to southern Italy, 33 to northern Italy, and 22 to Rome or Venice. Regarding age, 28 were under 15 years, 609 from that to 20, 513 from that to 25, 291 from the latter age to 35, while 59 ranged from the age of 35 to 40, and only 9 were older, but none exceeded 48 years. Again, 151 were married women, and 113 widows, the remaining 1245 being spinsters. In reference to instruction, 1330 were entirely devoid of any education, being unable to read, write, or figure; and to use the Italian designation of such ignorant persons, they were "Analfabete."

Respecting the causes which were assigned as leading those 1509 females to adopt the life they then followed, the report already quoted states, that in 369 excessive misery was the cause, in 290 loss of parents, in 180 bad example, in 127 cruel treatment by parents, concubinage in 58, avarice in 54, in 41 widowhood, and in 24 luxury, while in 250 cases seduction was the cause, the remaining cases being ascribed to other influences not sufficiently important to deserve specification. Among the above 1509 prostitutes, only 241 were healthy, the remaining 1268 being more or less diseased, of whom 501 had been once transferred to the syphilitic hospital, the other 767 cases several times; thereby indicating the great prevalence of venereal maladies, among a class of persons said to be constantly under rigid police surveillance, and subjected to frequent periodical medical inspection.

The additional points to which it appears worth to direct attention, are, among others, the question as regards pregnancy occurring among unchaste females. According to the report previously named, 150 then became pregnant; that is, one in every 10 public prostitutes had actually conceived, notwithstanding their dissipated life. Among the 150 cases of pregnancy, 66 ended in abortion, 36 were premature births, whereas 48, or only 32 per cent., went their full time; although, in these 48 cases, 13 bore dead infants, in 7 the child died very soon afterwards, while 28, or rather more than one half, survived. Consequently, from these statements, it may be assumed with confidence, if living so dissolutely women become pregnant, the chances are greatly against that condition attaining maturity, or their offspring being born alive, and even of living any time subsequently.

Finally, the reasons influencing women of the above class retiring from their mode of life likewise merit some notice. Thus, 313 hid themselves, or, in "Latitazione," as the Italian word expresses, 18 became concubines, 11 were licensed to keep a brothel, 8 condemned judicially, 6 married, 3 became insane, and 22 died. To these details it may be further added as curious, that only 88 appear to have resumed the paths of virtue, of whom 14 were placed in some charitable asylum.

Before alluding to other Italian universities having a medical faculty, it ought to be stated, that several populous and important cities are devoid of such educational institutions, although they possess many qualifications requisite to render establishments of that description highly beneficial to all classes of society. Among other cities without a university may be first named Florence, the modern capital of Italy. Milan also, a large city, which in fact after Naples takes the next rank, is also in the same predicament, notwithstanding it possesses the largest hospital throughout Italy, so large that it is able to receive 3400 patients, and generally has 2500 under treatment, besides having various lecturers, and numerous pupils in attendance.

Indubitably, there exists in Florence a practical school of medicine and surgery, somewhat analogous to Milan, having professors and pupils in connexion with the two Tuscan universities, Siena and Pisa, by whom degrees in physic are alone granted to medical alumni. Nevertheless, to obtain diplomas, the principal part of a student's professional education must be there procured; the curriculum required by the above institutions from graduates being three years' residence at either of the above-named universities, with not less than twelve months' attendance at the practical school in Florence.

Again, Venice, long an important wealthy city, and which still contains a large population, with an extensive hospital for patients attacked by disease, besides having other appliances well adapted to promote medical education, virtually has no university, Padua being still, as it has long been, the Alma mater of the anciently powerful province Venetia; or, speaking more correctly, the great republic which often carried on war against neighbouring states, kings, the pope, and sultan.

Contradistinguished from the three large cities just specified, where no university exists, insignificant places like Modena, Parma, and Perugia, which were formerly the petty capitals of very small provinces, but now only second- or third-rate towns, still possess such educational institutions. Doubtless, these learned corporations, as also several others of even less repute, might be suppressed, or perhaps transferred advantageously to localities much better adapted for educational purposes, viz., to Milan or Florence. Indeed, the idea of closing some of the smaller Italian universities, and thereby rendering the remainder more effective, already occupies public attention.

This transference of knowledge-imparting institutions from small towns to capitals, or important and crowded centres of modern civilization, seems now to be practised throughout Europe, whereby various state-centres, which not many years past were without universities, have recently been supplied, either by transferring an ancient institution to a capital, or by creating new educational and degree-conferring establishments in such localities. Thus, Madrid, Munich, Brussels, and Berlin, have been so treated in modern times, while Lisbon, Stockholm, Dresden, Amsterdam, and Florence, still remain without any properly constituted university. Nay, even London has no such really teaching institution, being dissimilar to Edinburgh, Dublin, and various continental capitals in that respect. No doubt, the University of London grants degrees to alumni taught elsewhere; but having no professors, and not communicating knowledge, like those cities just named, the great English Babylon still belongs to the former category, according to ordinary and time-honoured designation.

As previously remarked, although Naples University is the largest institution in respect of the number of students throughout Italy, still that of Bologna is the most eminent, being said to date as far back as 433 A.D. But this is doubted, since some authorities assert that Charlemagne was its actual founder, towards the end of the eighth century, and soon after the foundation of Pavia by that emperor. During many centuries, this venerable seat of learning enjoyed great reputation, and was often attended by many thousand students. Of late years, it has declined considerably in public estimation, and now scarcely numbers as many hundred alumni as it had of thousands in olden times; the average amount of students attending its various faculties being from 700 to 800 on an average.

Early in the current century, Tomassini, one of the first promoters of the contra-stimulant sect of medical practitioners, whereof Rasori was the founder, occupied a professor's chair in this university, while Borda—another zealous apostle of that doctrine—filled one at Pavia. Attached to the Bolognese University there is an extensive museum and a large library, besides other appliances essential for every similar institution; while, besides the general hospital, where medical students attend to see ordinary patients under treatment, there is a clinical hospital, having thirty-two beds distributed in small apartments, always filled with selected cases which are specially devoted to the university professors for the purpose of delivering clinical lectures at the patient's bedside. Being given in Italian, within hearing of the person most concerned, these prelections, some of which extend to considerable length, however instructive to juvenile practitioners may prove, we think, iinjurious to sufferng hearers, particularly when the professor is discussing existing dangerous symptoms, or enunciating a probable fatal termination to the malady under examination. Proceedings like these are highly objectionable; and therefore it would be much more judicious, and equally useful to pupils, were clinical teachers, after visiting and inquiring into the condition of their patients, invariably to lecture on the cases in some separate apartment.

Outside an adjacent city gate, but not far from the university, there is a small lunatic asylum, containing generally from 250 to 300 insane residents, at which students may also attend, in order to obtain practical instruction on the treatment of mental diseases. In short, this ancient Alma mater is well supplied with the requisites essential for imparting medical or surgical instruction; and if government cordially promotes academical reform here as elsewhere, which public rumour states they contemplate, the University of Bologna may very likely soon regain its pristine high reputation for learning, and especially will resume the eminent position it formerly occupied in the cultivation and teaching of medical science.

Pavia University is also an educational institution of great antiquity, and having been founded by Charlemagne in 774 A.D., it constitutes one of the oldest establishments of that description now existing in Italy. It has 35 professors and 21 assistants, with the customary number of faculties, while the average number of students now ranges about 1450, which however exceeds considerably the amount of pupils attending while the "Tedeski" ruled in Lombardy. This school has long been celebrated for its superior medical and surgical teaching, besides being eminent in several branches of learning; Spallanzani, Volta, Scarpa, Panniza, and many other distinguished men of science, having filled professorial chairs. The University Library is extensive and valuable. There are also well-stored museums of natural history, anatomy, and pathology, all being made available by students for acquiring knowledge, or used by the various teachers when lecturing; while the General Hospital, with clinical wards, supply ample opportunities for obtaining practical experience respecting the nature and treatment of disease, whether medical, surgical, or obstetric. In short, this "Alma mater" still retains its high reputation as a school of medicine; and now having a good revenue, it has become much more efficient than during Austrian domination.

Padua likewise possesses a University; not so old, certainly, as either of the two analogous institutions previously mentioned; but one which, however, enjoyed great, if not more, celebrity at the beginning of the twelfth century than any other Italian educational establishment. Such was the case especially during the sixteenth and seventeenth centuries, when some of the greatest medical authorities in Italy were professors belonging to the Paduan school, along with whom Galileo may be named, he having filled the chair of mathematics during many years at this university. Eminent foreigners often frequented the medical School of Padua to study their profession. Of these may be mentioned Harvey; and here it is said he first commenced those investigations respecting the circulation of the blood, which ended in his immortal discovery. The Paduan University has the customary five faculties, comprising forty-six professors; whilst the average annual number of pupils attending classes recently ranged from 1700 to 1800, the largest proportion being medical.

Adjacent to the university is the General Hospital, containing usually about 350 patients. It has four clinical departments for instructing university students, viz., medical, surgical, obstetrical, and ophthalmic—diseases of the eye being here very numerous: indeed, they are very prevalent throughout most districts of Italy; and hence ophthalmic surgery is much cultivated at this school, as elsewhere.

Although the university library, museums, and other adjuncts are good, they cannot be compared with those at Pavia in reference to extent; nevertheless, most seem well adapted for teaching purposes. Doubtless, after it has enjoyed a government which the peolpe like, Padua will accomplish both extension and improvements. Moreover, a veterinary college is attached to the university, having teachers, Professor Panniza being director. This institution is extensive, has an appropriate museum for instructing pupils in a knowledge of the anatomy, pathology, as also the treatment of diseases prevalent among horses; and altogether this department may be deemed well adapted for the objects proposed.

Besides the brief notice already made respecting the general hospital, it is further interesting to add that, this establishment may be classified as one of the best constructed receptacles for sick persons throughout Italy. Being a modern structure expressly built as an hospital, and unlike most Italian establishments of that description—which often formerly were old convents, and hence ill-adapted for persons labouring under disease or suffering from accidents—this building forms a square, having a garden in its centre, and open arcades all around. The former enclosure is used by convalescent patients for enjoying out-door exercise; the latter for a similar purpose during rainy or stormy weather, as likewise to enable officials and others to perambulate the establishment under cover, whether during rain, sunshine, or otherwise. Airy dormitories occupy the first and second stories, each sex being located in separate floors, while that on the ground is appropriated for offices, magazines, and so forth. Hence, the interior arrangements of this charitable institution being well adapted for the special purposes proposed, and its architecture exteriorly also possessing a pleasing aspect, the General Hospital of Padua may be justly pointed out to other Italian municipalities, as presenting a good model, in various respects, for imitation.

Other Italian Universities having medical faculties of some repute besides those already adverted to, specially might be mentioned, namely, Turin, Genoa, and Palermo; particularly the former, where the students are nearly as numerous as at Pavia; but they need not occupy attention any more than the four universities only alluded to nominally in previous paragraphs: while the remaining four, viz., Catania, Sassari, Messina, and Cagliari, seem of such insignificant importance, and have so few students, that they are only now specified so as to complete the catalogue of sixteen, at present in operation, and supported by the existing Italian government.

Two important facts should, however, be stated in connection with Italian educational institutions. In addition to other judicious improvements, seven new professorships have very lately been instituted at seven different universities, the occupants of which are to lecture on mental disease, and also illustrate their treatment by cases in public lunatic asylums. Innovations of that character are highly laudable, and will prove most beneficial as well to future medical practitioners as to humanity. The other point adverted to, although wholly different, is, however, significant: seeing it raises the status of university professors, and hence often, also, of medical practitioners. The matter in question being that, not only are such officials in various instances elected as members of the chamber of deputies, but the Sovereign has created various professors in the medical, as in other faculties, Senators, or peers for life, who hence constitute part of the legislature.

Several examples of medical Senators might be named; but it will suffice to quote, first, the late eminent rector of Pavia University, Dr. Panniza, who was formerly professor of anatomy in that school; and, second, the distinguished Florentine surgeon, Il Signor Buffalini. Besides these illustrations of honours conferred upon Italian members of the healing art, it may be added, both physicians and surgeons have been nominated to important public non-professional appointments; nay, sometimes made ministers, and even premiers, as exemplified by the late Dr. Farini.

In other continental countries analogous instances of honours paid to eminent medical men might be also quoted. Thus, in Spain, Queen Isabella's physician-accoucheur was created a marquis and hereditary peer. Napoleon III lately made his confidential physician a Senator; while in Sweden Dr. Ekströmer, physician to the king, was ennobled, and now sits in the House of Lords of that country. Rarely have English practitioners of physic attained similar marks of distinction. Lord Glenbervie was no doubt created an English peer, although a member of the profession; but then, having previously changed his calling and become an eminent diplomatist, this advancement to the peerage was in reward of great services so rendered. Again, into the British House of Commons few medical men have effected an entrance, while, at present, there are only six gentlemen of that category entitled to write the highly significant and much coveted letters "M.P." after their names.

The Eternal City, Rome, has 'La Sapienza," which is considered a university; while there are several hospitals where students are taught various branches of medical science, who afterwards become licensed to practise their profession.

"Il Collegio della Sapienza"—the University of Rome—was founded in 1244; but then chiefly as a school for the common and civil law. In 1825 this college was entirely remodelled. It comprises five faculties, like other Italian universities, and has forty-two professors, of whom thirteen are medical. The total number of pupils range usually about 600; but a rather small proportion study medicine. The college has a library, museum, and botanical garden. However, if compared with most analogous Italian institutions, this establishment will certainly be deemed, as a medical school, very inferior.

Few European cities are so amply provided with hospitals for receiving sick patients, and hence available for studying medicine or surgery like Rome. Many of its public charitable institutions are hihgly endowed, especially the hospital "Spirito Santo" near St. Peter's, which is the largest for treating inmates labouring under disease in the Roman metropolis. Only male patients are received at this charity, which can accommodate, in ordinary times, about 500 medical and surgical cases, although when endemic intermittent fever or cholera has prevailed, more than double that number of inmates have been then admitted. Very recently considerable improvements were accomplished. But, being close to the Tiber, its site is considered unhealthy; while the ventilation proves often defective, especially when a damp fog arises from the river immediately adjacent. This offensive emanation is by no means infrequent, and sometimes becomes so dense and unhealthy that during early morning hours, even in the forenoon, to open windows for admitting fresh air within, or to so allow the ward's deteriorated atmosphere any free escape outwards, would be deemed dangerous by resident officials.

Adjoining the Spirito Santo Hospital, indeed forming part of that establishment, there is an asylum for lunatics, which can receive about 450 inmates. To this establishment large gardens on an adjacent hill have very lately been added, where patients of a non-pauper class are admitted, while open air labour can be thus supplied for those lunatics who would benefit thereby. Spirito Santo likewise comprises a department for foundlings; but much to the credit of government, the wheel for receiving "Depositi" is no longer in operation, it being recently enacted that foundlings shall henceforth not be admitted secretly, 'or through unrecognised parties, or by persons unknown. This important reform all must applaud; and the same rule ought to be adopted in every country, where foundling hospitals still continue managed, under their old most objectionable regime.

Near the "S. Giovanni di Laterano Basilica" the hospital "Santissimo Salvatori" is appropriated for sick and aged females. It can admit about 600 patients; but although a little better ventilated than some other Roman charities, and even situated on rather elevated ground, the locality occupied being considered insalubrious, its mortality usually exceeds that recorded in some other similar institutions. This result may also in part arise from the wards being often too crowded, which sometimes contain four rows of beds in one large apartment. But as that bad custom, however, often prevails elsewhere, the consequent injurious effects are thereby less recognised, and hence such overcrowding of patients becomes overlooked by hospital authorities.

"La Consolazione" Hospital, on the Capitoline declivity, also merits notice. It only admits surgical cases, especially accidents, but without distinction of sex; and being situated near the Monti and Trastevere quarters, where stiletto attacks oftenest occur amongst the population, parties so wounded are usually taken thither. The wards of this hospital are clean and airy: they contain about 100 beds, and the situation being healthy, that feature therefore contributes materially towards a patient's recovery.

The "S. Giacomo" Hospital in the "Corso," or Regent Street of Rome, is, properly speaking, a Lock Hospital for both sexes, and can accommodate about 350 patients, upwards of 2000 cases being the usual annual admissions. The male wards, or upper floor, are clean and large, but not well ventilated. Recently the building has been enlarged, and some improvements effected; while much of the ancient portion was lately closed, in consequence of inherent defective arrangements, and its dungeon-looking character. Although the internal management and economy of this charity is superior to that generally pursued in the three Roman institutions already named, and its cleanliness greater, the situation occupied is not good, being too near the fashionable promenade, while the ventilation might be still better managed than at present.

Besides the four public hospitals in Rome for sick patients above designated, and where students may attain professional knowledge, that of "S. Giovanni dei Benfratelli" should be added. It is situated in an island of the Tiber, and curiously enough on the site occupied during ancient times by a temple dedicated to Esculapius. Into it only male patients affected with acute diseases are received, and the number of beds ranged lately under seventy-five, being however rarely all occupied.

The "S. Gallicano" is another hospital in "Trastevere" appropriated to persons of both sexes suffering from cutaneous complaints; it can accommodate 240 indoor patients, and has two large wards, which are well ventilated.

Further, Rome also possesses an hospital, chiefly used for convalescents, called "Santa Trinita dei Pelligrini," and is near the bridge "Quattro Capi." To this institution poor patients are removed from those named in previous pages, where they may remain three or more days to complete recovery, and afterwards receive clothing, if necessary, on leaving for home or otherwise. Finally, a small lying-in hospital near the "Ripetta"—Port of Rome—called "S. Rocco," must not be omitted; more especially, as it is the sole public establishment of the kind now existing in this metropolis.

Nevertheless, and notwithstanding the extensive means available for students obtaining practical experience, both in medicine and surgery, Rome cannot compete as a medical school with many Italian universities, either in respect of scientific knowledge or efficiency; such criticism regarding its inferiority being especially applicable in reference to midwifery, if not also to ophthalmic science, whether taught by lectures or through clinical illustrations. In support of this opinion it may be stated, there is no obstetric professor at the "Sapienza," and even the Rocco lying-in establishment has only twenty beds, while great precautions are adopted to ensure secresy, as also not to hurt the feelings, or injure the fair fame of parties treated therein, hence it follows that medical pupils really have few opportunities of obtaining useful information, or practical experience on that important department of professional knowledge, which always constitutes an essential portion of medical education at every school or university, where the art and science of healing is now taught. But here, at the Sapienza, &c., in various other respects, decided reforms must be accomplished, so that educational defects, whether professional or otherwise, shall be effectually remedied.

Having given in previous pages an outline of the present condition which Italian medical schools in connexion with universities at present exhibit, and likewise alluded, although generally, to several public hospitals, where practical experience could be obtained through personal observation, or students taught by clinical lectures, an important class of institutions containing mentally diseased inmates must be noticed, whose maladies, as also treatment, well deserve being studied by medical pupils, namely, the existing lunatic asylums of Italy. But, further, those now being erected merit remark, as also certain proposals for new insane establishments, of which several Italian provincial authorities contemplate the construction.

Analogous to most European countries, until very recently, Italian lunatic asylums were usually erected within the precincts of cities, or in their immediate vicinity; while all buildings containing insane patients more frequently resembled prisons, than dwellings intended for human beings, especially when afflicted with mental derangement. Among existing public institutions in Italy for lunatics, the asylum of Turin, designated "Il Regio Manicomio" may first be noticed. It was built about thirty-five years ago by an architect who knew very little, or indeed nothing respecting lunacy; and, as he would not deign to consult any competent medical authority, regarding the proper construction of institutions necessary for insane residents, the present structure is very badly arranged, being wholly inconistent with properly lodging, or judiciously treating, and still more usefully employing lunatic inmates. Few adequate means, or indeed almost none whatever, being available wherewith to occupy patients, their time is chiefly consumed by playing cards, walking about occasionally in small adjacent court-yards, or idly perambulating the large, ill-ventilated, and often crowded galleries of the establishment. Sometimes patients may read; but indeed this institution cannot be compared with many French or foreign insane receptacles, or even with several more recently constructed Italian asylums. The ordinary number of residents in the Turin Manicomio ranges about 850, among whom males predominate, while restraint in both sexes is often employed. Connected with this asylum a succursal has been recently established, to which convalescent male patients are often sent, as well to complete recovery, as to afford useful employment in the open air, but chiefly in garden labour or agriculture. Being only a few miles distant from Turin, in a salubrious and pleasantly situated district, this appendage has proved an advantageous addition to the parent establishment.

Milan, which possesses one of the largest general hospitals in Italy, viz., "Il Ospital grande," with a staff of at least 100 medical officers, already mentioned, has also a lunatic asylum, "La Senavra," situated near one of the city gates, and occupying a low, damp, insalubrious locality. The building was originally a convent of Jesuits, and hence is quite incompatible with its present destination. Most likely the establishment will be suppressed or moved elsewhere, which would prove highly judicious, as the defects of structure are almost irremediable. It usually contains about 500 lunatics, the sexes being generally equal in number; and all are paupers. As in Turin, physical coercion is here often employed. In fact, throughout most Italian lunatic asylums, the non-restraint system has made by no means the same satisfactory progress as that now frequently witnessed in more western European countries. Still, non-coercive treatment has made advances in Italy during recent years; and at an asylum afterwards to be favorably noticed, strait-waistcoats and similar appliances are almost unknown, or have become mere relics of history.

About ten miles from the capital of Lombardy an additional public asylum for lunatics has very lately been fitted up and opened, which may be truly called not only the best in Italy, but quite equal to many of the most approved institutions for insane residents in France or Great Britain. This manicomio, named "La Casa di Mombello," was formerly the patrimonial residence or palace of the Marquis Crivelli, and occupies an elevated position in a beautiful fertile country near Monza; whereby, from every side it enjoys an extensive prospect, with a long range of the alpine, and often snow-clad mountains northward. Altogether, this asylum is placed on almost one of the finest sites in Europe; and what seems worth mentioning, if not also historically curious, when Napoleon I commanded in Italy, he resided in this palace during some months prior to the battle of Marengo; while, further, his two sisters, the Princesses Borghese and Bacchioci were both married here with splendid pomp and ceremony. The great conqueror's bed-room, when visited by the narrator of this singular alteration, was then the dormitory of seven insane pauper occupants.

At the Mombello Asylum 300 patients can be accommodated, and these are principally occupied in out-door, free-air employments, that is, for cultivating the extensive gardens, or in agriculture; while handicrafts and trades wherewith inmates had previously been familiar, and still adapted for their particular condition, are likewise zealously encouraged. Consequently, this new establishment for the insane deserves much commendation.

Analagous to the capital of Lombardy, Venice has also its lunatic asylums—one for male patients on the islet of "San Servilio," another, appropriated to insane females, occupying a more confined position in the city, and forming a department in connection with the Municipal Civil Hospital, for all classes of disease or accidents. The "San Servilio" asylum is one of the best managed in Italy, and will repay being visited by foreign physicians, who have often expressed approval of the system there pursued, as likewise of the humane treatment which inmates receive from the administration.

The lunatic asylum at Perugia likewise deserves notice, having acquired much celebrity in Italy. This "Ospedale de Menticatti" lies outside one of the city gates, and contains both pauper inmates and paying patients. The number under treatment is not considerable, being usually about 100; but the whole establishment, being extremely well-managed, merits a visit from professional travellers and philanthropists. The system of non-restraint, although less pursued here than is usual in England and some foreign insane establishments, still has been productive of excellent results.

Besides Aversa and Palermo, the Lunatic Asylum of "San Niccolo," at Siena, must not be forgotten in this general sketch of Italian institutions for the insane. This latter "ospedale" was formerly an old convent, and consequently not well adapted for its present object; but, having been greatly improved in its arrangements, it is now much better suited than when first occupied. Large additions are further contemplated. Indeed, some have been already effected; while more are decided upon, which, if carried out as proposed, will virtually render this well-managed institution one of a superior description. Its site is salubrious, and it forms an elevation near one of the city gates, and enjoys fine prospects of the undulating fertile country adjacent. In short, the situation is altogether excellent. The average number of insane patients range about 200, the sexes being about equal. A large proportion of both are usually employed in out-door occupations, or in handicrafts within. Restraint of any kind is very rarely employed; and, in truth, the present writer may add, he did not notice a single example of restraint in this asylum, although frequent inspections were made during a residence in Siena of nearly one month. Consequently, it may be stated with justice that "San Nicolo Manicomio" is exceedingly well conducted, and highly creditable to Dr. Levi, its accomplished medical director.

The only other public institution for lunatics to which it is proposed directing attention, is the "Ospedale di Bonifazio," at Florence, situated within the city precints, but in a position specially objectionable. Although originally founded in 1377, the existing building dates from a more modern period. In general arrangements, adaptability for treating lunatics, or otherwise, it must be ranked as being even inferior to the Milanese Asylum of Sinavra. Restraint still prevails to some considerable extent at this institution, and the wards are badly constructed; while its ventilation requires much amelioration. Both paying and pauper lunatics are admitted into this establishment, which generally contains from 400 to 450 inmates; the sexes being about equal in number. Having been condemned by medical and public opinion, as not adapted for properly treating insane patients, it is proposed to remove the entire establishment elsewhere, as soon as sufficient funds shall be forthcoming. Indeed, a rumour prevails in Florence that government intend converting the "Palazzo Ricardi," with some adjoining fields occupying a country district, into an asylum where various modern appliances for treating the insane, used elsewhere, and other important improvements not easily available at the present locality, will be put into operation; whereby it may be rendered more beneficial for afflicted inmates, and conformable to existing psychological science.

In every part of Italy, with the exception of Rome, periodical literature and printing generally is quite free and unrestrained. This appears most evident at university and populous towns, where pamphlets on various subjects, periodicals, medical and of other kinds, seem abundant, along with publications of a scientific and progressive character, which may now make their appearance without governmental hindrance. Political questions being the predominant subjects which, at present, attract most attention and cause discussion, medical literature has scarcely manifested the same activity which some other departments have shown, or what it will doubtless exhibit when several subjects now attracting much public attention shall be neglected and forgotten. Nevertheless, medical works of value have lately issued from the Italian press, of which several merit attention from transalpine practitioners.

British medical men, who are often cognisant of French and German medical literature, should therefore extend their acquaintance with new professional works now issuing from the press of a country which has produced Baglivi, Morgagni, Scarpa, Volta, Palletta, besides many other Italian celebrities both ancient and modern. In our estimation—derived from experience—readers would often obtain valuable information by adopting such recommendation; nay, occasionally get practical knowledge of great value.

Although not much known in other countries, or even among natives, as possessing medicinal springs, which will prove useful in the treatment of disease, still Italy has various thermal establishments and fountains whose waters are endued with considerable therapeutic virtues. Of course, the Italian peninsula cannot be compared in that respect with France, Spain, or Germany. Nevertheless, there are some Italian localities deserving notice, to which we wish now to direct attention, even if but briefly, in order that the springs of repute may be better understood by English physicians than heretofore.

Even when the Roman Empire governed the then civilised world several places having medicinal waters were well known for their curative efficacy in disease, both in Italy and throughout other districts of Europe; when the custom of frequent baths and drinking mineral waters was often adopted for various maladies, or merely as an indulgence, and sometimes only to gratify the frequenter's luxurious propensities.

Speaking generally, the mineral waters of Italy may be divided into three zones; namely, those met with in the northern part of that Peninsula, those of central Italy, and lastly the springs found in the southern districts: of which latter, the chief belong to Naples and its vicinity.

The baths of Aqui in Piedmont may be first mentioned. It lies near the town of Alexandria, and has sulphurous waters, which are both cold as well as warm, the latter being known by the name of "Bulliente." This water usually reaches the temperature of 158° Fahrenheit, is very limpid, exhales a sulphurous odour, and contains salts of iron, lime, and alum, with a little silex. Besides the Aqui water, the mud found lying in the right bank of the river Bormida, about a mile distant, is also employed extensively, as is the custom at Marienbad; but in temperature this product ranges much lower than the water of Bulliente. Moreover, the medicinal water of "Masino" in the Valtellina has much reputation in uterine diseases, dysmenorrhœa, leucorrhœa, and other maladies affecting females. Indeed, its therapeutic action is so marked that this place is called the *Ladies' Bath*. Again, the salino-iodic fountain of "Miradola," near Lodi must also be named among the mineral medicinal springs of northern Italy; as likewise that of S. Vincenzo, near Aosta, which, being acidulo-ferruginous and saline, proves diuretic, lithontriptic, and deobstruent, and consequently, as such, is often beneficial.

The ferruginous spring of "Levico," between Trent and Bassano, also in North Italy, at present enjoys considerable therapeutic reputation, in consequence of being the water which contains more iron in its composition than any similar fountain throughout high Italy. This is the case both at the "Caverna del vitriolo," where the water is employed for bathing purposes, as also in the water obtained from the "Caverna dell'Ocra," which is only used as a beverage. The bath of "Comano," likewise in the Trentine district, was even known to the ancient Romans; but it continued long as lost, until early in the current century, when the spring was again discovered. The "Comano" water resembles that obtained at Plombières, in France, and is hence employed chiefly in cutaneous affections. Finally, the celebrated fountain of "Beccaria," lying west of "Vicenza," was first discovered in 1694, and in chemical composition it resembles that of Spa and Pyrmont. From this similarity, medical authorities consider the Beccaria spring as frequently proving useful in cutaneous diseases, and for chronic gout; while recent experience has demonstrated the efficacy of its water being so employed.

In the central districts of Italy, or rather in Tuscany and its vicinity, the baths of "Porretta," may be first mentioned from lying at the foot of the Appennines, between Bologna and Pistoia, on the banks of a mountain torrent named "Il Rano." The water of this spring contains sulphur, soda, lime, and a very little iodine; being also slightly gaseous. Its action is diuretic and purgative.

may be classified as one of the best constructed receptacles for sick persons throughout Italy. Being a modern structure expressly built as an hospital, and unlike most Italian establishments of that description—which often formerly were old convents, and hence ill-adapted for persons labouring under disease or suffering from accidents—this building forms a square, having a garden in its centre, and open arcades all around. The former enclosure is used by convalescent patients for enjoying out-door exercise; the latter for a similar purpose during rainy or stormy weather, as likewise to enable officials and others to perambulate the establishment under cover, whether during rain, sunshine, or otherwise. Airy dormitories occupy the first and second stories, each sex being located in separate floors, while that on the ground is appropriated for offices, magazines, and so forth. Hence, the interior arrangements of this charitable institution being well adapted for the special purposes proposed, and its architecture exteriorly also possessing a pleasing aspect, the General Hospital of Padua may be justly pointed out to other Italian municipalities, as presenting a good model, in various respects, for imitation.

Other Italian Universities having medical faculties of some repute besides those already adverted to, specially might be mentioned, namely, Turin, Genoa, and Palermo; particularly the former, where the students are nearly as numerous as at Pavia; but they need not occupy attention any more than the four universities only alluded to nominally in previous paragraphs: while the remaining four, viz., Catania, Sassari, Messina, and Cagliari, seem of such insignificant importance, and have so few students, that they are only now specified so as to complete the catalogue of sixteen, at present in operation, and supported by the existing Italian government.

Two important facts should, however, be stated in connection with Italian educational institutions. In addition to other judicious improvements, seven new professorships have very lately been instituted at seven different universities, the occupants of which are to lecture on mental disease, and also illustrate their treatment by cases in public lunatic asylums. Innovations of that character are highly laudable, and will prove most beneficial as well to future medical practitioners as to humanity. The other point adverted to, although wholly different, is, however, significant: seeing it raises the status of university professors, and hence often, also, of medical practitioners. The matter in question being that, not only are such officials in various instances elected as members of the chamber of deputies, but the Sovereign has created various professors in the medical, as in other faculties, Senators, or peers for life, who hence constitute part of the legislature.

Several examples of medical Senators might be named; but it will suffice to quote, first, the late eminent rector of Pavia University, Dr. Panniza, who was formerly professor of anatomy in that school; and, second, the distinguished Florentine surgeon, Il Signor Buffalini. Besides these illustrations of honours conferred upon Italian members of the healing art, it may be added, both physicians and surgeons have been nominated to important public non-professional appointments; nay, sometimes made ministers, and even premiers, as exemplified by the late Dr. Farini.

In other continental countries analogous instances of honours paid to eminent medical men might be also quoted. Thus, in Spain, Queen Isabella's physician-accoucheur was created a marquis and hereditary peer. Napoleon III lately made his confidential physician a Senator; while in Sweden Dr. Ekstrômer, physician to the king, was ennobled, and now sits in the House of Lords of that country. Rarely have English practitioners of physic attained similar marks of distinction. Lord Glenbervie was no doubt created an English peer, although a member of the profession; but then, having previously changed his calling and become an eminent diplomatist, this advancement to the peerage was in reward of great services so rendered. Again, into the British House of Commons few medical men have effected an entrance, while, at present, there are only six gentlemen of that category entitled to write the highly significant and much coveted letters "M.P." after their names.

The Eternal City, Rome, has 'La Sapienza," which is considered a university; while there are several hospitals where students are taught various branches of medical science, who afterwards become licensed to practise their profession.

"Il Collegio della Sapienza"—the University of Rome—was founded in 1244; but then chiefly as a school for the common and civil law. In 1825 this college was entirely remodelled. It comprises five faculties, like other Italian universities, and has forty-two professors, of whom thirteen are medical. The total number of pupils range usually about 600; but a rather small proportion study medicine. The college has a library, museum, and botanical garden. However, if compared with most analogous Italian institutions, this establishment will certainly be deemed, as a medical school, very inferior.

Few European cities are so amply provided with hospitals for receiving sick patients, and hence available for studying mediwater also acts beneficially against leucorrhœa, blenorrhœa, and other diseases of debility, where astringents are indicated.

Near the town of Pozzuoli, about four miles west of Naples, the mineral waters of the temple of Serapis comprise two kinds, viz., warm and cold. The first embraces the "Acqua dell' Antro," and "Acqua della Marchina;" the cold springs being named "Acqua Media," and "Acqua dei Lipposi." The two former, or cold, are in composition analogous to Toplis, the other, or warm, resembling Ems. Respecting their medicinal properties, the Antro and the Marchina waters taken internally act as a tonic and mild aperient in dyspeptic diseases, gouty diathesis, visceral obstructions, and habitual constipation; while as a bath they are useful in rheumatic affections, lumbago, sciatica, stiffness of joints, herpetic or cutaneous diseases, and in scrofulous constitutions. The cold, "Acqua Media," may be used like the two former-named waters, but that of Lipposi is principally employed as a collyrium.

The remaining thermal waters of South Italy which decidedly merit notice, are those belonging to the Island of Ischia, in the Bay of Naples, and not far from its north-west shore. The springs there are numerous and exceedingly valuable; that of "Gurgitello" occupying a high position in public estimation, while its temperature rises sometimes to 138° Fahrenheit. In chemical composition the Gurgitello water bears some analogy to that of Carlsbad, and contains muriate, as also bicarbonate of soda, bicarbonates of lime and magnesia, sulphates of soda and lime, besides a little silica. Hence it is ranked as both gaseous and saline. In therapeutic action the water is aperient, and also affects the cuticular circulation: being frequently used for baths, and taken also as a beverage. In lymphatic and scrofulous constitutions this thermal spring acts beneficially, while for paralysis of the lower limbs it has obtained great reputation, as likewise for osseous caries. Against affections of the nervous system, and for persons having excitable temperaments, the water is not found so useful, and when used it should be employed more cautiously. In uterine disease, rheumatic affections, chronic inflammations, the sequelæ of gout, and in dropsical effusions, the Gurgitello water also frequently proves beneficial. Besides which, parties whose health has suffered through residence in hot climates will find it advantageous.

The "Acqua di Cappone," another Ischian spring, rises within a short distance of the former, and issues in great abundance. It has the smell of weak chicken broth, and is generally supposed to have derived its name from that resemblance. In chemical composition the Cappone water is analogous to that of Gurgitello, but proves much less powerful, and is considerably lower in temperature, ranging about 98° Fahrenheit. Regarding its therapeutic properties, being diuretic and moderately aperient, it resembles the former water in the treatment of disease. It is further especially reported useful in cases of sterility, for which condition this water was formerly much celebrated by Jacobin.

The "Acqua Citara," found in a small bay on the western side of this island, has also acquired still more celebrity than the former for its great efficacy, according to popular belief, in obviating barrenness. Even during ancient times the Citara spring was reported to possess heroic virtues towards promoting female conception, its name having been actually given in honour of the goddess Cythera. However, be such derivation true or otherwise, this medicinal fountain still retains great repute in uterine maladies, and also respecting the effect it is assumed to exert towards increasing population. Indeed, this faith is so strong that, report asserts, young women anxious of becoming mothers still confidently resort to Citara, in order to ensure such desired contingency.

In leucorrhœa, chlorosis, dysmenorrhœa, and debility of the digestive organs, the water also frequently proves efficacious: while for obstructions of the abdominal viscera, being powerfully aperient as also diuretic, it is then employed with advantage. Acting, further, as a tonic and stimulant in debility arising from miscarriages, for affections depending upon nervous irritability, and against hysteria, the Citara water will sometimes produce excellent results. But the high renown of this very celebrated spring rests chiefly on its reputation in cases of sterility; and that some foundation for such a character exists seems proved by the reputed experience of ages. Indeed, the baths of Ischia have enjoyed much repute during more than 2,500 years; it being even said the waters were employed for medicinal purposes several centuries prior to the time of Hippocrates; and as he was born 460 years before Christ, the mineral springs of Ischia, consequently, are the most ancient throughout Europe.

Besides the sources above designated, there are numerous others in this locality, whose names it seems only necessary to mention, as indicative both of the abundant supply and variety of these natural productions. They are the Acqua dell Vecchio, Sperma Polastro, Colata, Couva, Sinigalla, Ferratez, Rivaz, Tamburo, Verde, Castiliglione, Fontana, Fornello, Pontana, Rita, Santa Restituta, Isabella di Borbone, San Montana, Capitello, Francesco primo, Œmitello, and Nitroli. In fact, the small island of Ischia overflows with medicinal waters, and hence invalids in search of health, through the aid of such remedies, will here find ample varieties of these therapeutical appliances, whether administered in the form of baths, douches, or taken internally as beverages.

REVIEW VII.

Kidney Diseases, Urinary Deposits, and Calculous Disorders; their Nature and Treatment. Containing Seventy Plates and Tables for the Clinical Examination of Urine. By LIONEL S. BEALE, M.B., F.R.S., &c. Third edition, much enlarged, 8vo, pp. 472.

THE profession is greatly indebted to Dr. Beale for his persevering researches into the histology both of healthy and diseased tissue, and for the indomitable industry which he exhibits in discussing, publishing, and illustrating the facts he arrives at from his own investigations, as well as those communicated by other original workers. The present volume, both in its text and in its illustrations, is a proof of this. The text displays his wide acquaintance with the views and experiments of others, as well his own countrymen as foreigners, whilst nearly all the analyses have been made by himself, and the drawings from preparations have, in most cases, been copied by him on the blocks, which were afterwards engraved. At the same time it is right, in dealing with this book as a new edition, to notice that much of its contents have appeared in previous publications of the author, and that it may be in great measure regarded as a collected edition of his several treatises on renal diseases and urinary deposits. The ground-plan or basis of the volume is to be found in the lectures on the urine, delivered in 1852, and subsequently published in the 'British Medical Journal,' and again in 1861 collected and re-edited by the lecturer himself in a separate volume. Of this work, the present is professedly an enlarged edition, in which several of the articles have been rewritten, and the more recent facts and hypotheses, physiological and pathological, introduced, illustrated, and discussed.

But throughout, the original plan of dealing with the subject-matter of the treatise has been preserved, a fact thus referred to in the preface to the first edition, which is still retained as declaratory of the scope of the present work.

"The course on urine included oral lectures and practical demonstrations, in which every pupil performed the experiments with his own hands, according to the directions given in the tables (found at p. 440 of the present work). . . . I have endeavoured (in these lectures) to restrict myself, as far as possible, to those parts of the subject which are of practical importance in investigating the nature of a case. It must be borne in mind that the lectures were given to practitioners, most of whom had far larger experience in practice than myself. Little advantage, therefore, could have resulted under these circumstances from discussing special questions connected with the treatment of disease, and almost the whole time was devoted to the practical examination of the urine and urinary deposits by the microscope, and by applying the appropriate tests. I have thought it right to retain this character in the present work, and only a few general remarks will be found with reference to the treatment of urinary diseases."

Since Dr. Beale lectured as a chemist and microscopist on the urine in 1852, and diffidently referred to his position as a youthful practitioner, he has enjoyed, as an hospital physician, wide opportunities for observing the history and treatment of urinary diseases, and consequently his remarks upon treatment, although still not extensive and too general, must bespeak attentive consideration.

It will, indeed, be generally regretted that the pathological portion of the treatise does not more thoroughly represent the experience of the author; or, in other words, that it does not partake in a larger degree of a clinical character, and so address itself to the wants of the busy practitioner in search of the wherewith to enlighten him in the understanding of the often complicated case before him, and in the best methods of dealing with it. To such individuals the accurately drawn pictures of disease in systematic works are but like the lay-figures of artists—artificial, rigid, lifeless—that can never be substituted for the study of the living reality, nor ever make the individual who relies upon them a successful worker.

In another direction, again, and even to a much greater extent, the original plan of the work has been enlarged. We allude to the physiological portion, which here assumes considerable importance, and is largely occupied with the enunciation of those views of minute structure and function with which Dr. Beale's reputation as a physiologist is so much identified.

To complete this general sketch of the characteristics of this new edition, one more quotation from the preface will suffice.

"The usefulness of the book has been increased by the addition of nearly one hundred new drawings, and as many more have been re-arranged in order in order to render them more truthful representations of nature; as in my other works, scales have been appended to the illustrations by the aid of which every object represented can be accurately measured without difficulty."

The work is divided into four parts:—(1) The kidney and its diseases; (2) Healthy urine and its examination; (3) Urine in disease and its examination; (4) Urinary deposits and calculous disorders.

With respect to the minute structure of the kidney, Dr. Beale considers Bowman's views to have been in all essential particulars confirmed by recent observations. He admits, with Virchow, the existence of *vasa recta* coming directly from arteries, but cannot accept Henle's statement of the presence of a distinct system of closed tubes connected with the Malpighian bodies. These canals are, indeed, nothing else than convolutions of the urinary tubes, so curved as to be reversed in direction relatively to the cortex for a certain length.

In controverting Henle's views, Prof. Beale administers correction to reviewers and writers in periodicals as a wrong-headed race, given to propagate histological and physiological heresies; complaining that they promulgate the theories of observers without first examining them for themselves in detail by preparing the necessary specimens, and carrying out the requisite experiments. But after the castigation we confess to not feeling very guilty, believing the periodical press to be well employed in giving currency to the observations and hypotheses of distinguished observers, whether such be opposed to, or in accordance with, received opinion. Indeed, in one important part of their functions, the recording and analysing of researches and hypotheses, their usefulness would be sacrificed by the waiting for time and opportunity to test for themselves the truth and groundwork of facts and opinions promulgated unceasingly. Their first duty is, we take it, to place all such before the scientific world as problems to be proved or disproved, and to await the results of inquiry. At the same time the journalist may find occasion to criticise views advanced, even when occasion is not afforded him to verify his objections, or he may happily be in a position at once to demonstrate their error. But enough of digression.

Dr. Beale uses very dexterously his ingenious hypothesis of *germinal* and *formed material* in place of the old cell-doctrine, in explaining the minute anatomy and physiology of the kidney. It is an hypothesis which is available for the elucidation of many intricate histological questions, and derives support from the study of the lowest animal and vegetable organisms. In the normal epithelial cell, says the author, both of the liver and of the kidney, there is no such thing as a cell-wall. There is nothing more than a central germinal mass surrounded by granular *formed* material, the former constantly reproducing itself and being transformed into the latter, which, on its part, is kept from accumulating indefinitely by the disintegrating process involved in the act of nutrition, with the activity of which, consequently, it stands in inverse ratio.

This doctrine of cell nature and growth has an important bearing upon the question of the presence and extent of connective tissue. As is well known, the connective tissue has been held by Virchow, and by many subsequent disciples of that eminent pathologist, to play a most important part in the history of disease. In Dr. Beale's conception, however, this tissue is of small account pathologically. All such in the kidney, where it constitutes the matrix, is represented by a very small amount of perfectly transparent tissue, which connects together the tubes and vessels. These last, indeed, are so disposed as not to need a supporting tissue, and in the embryonic state, when the tissues are softest and support most needed, no fibrous matrix is discoverable.

Moreover, the little interstitial tissue present in mature life is not active in disease—*e. g.* as in inflammation, but only passive; the active phenomena attributed to it being—

> "Really due to the presence of particles of germinal matter which have been detached from the white blood-corpuscles, and have passed through the vascular walls into the meshes of the connective tissue, where they grow and multiply very quickly,"

and give rise to granular or pus-corpuscles, or allied morbid products, as the case may be.

The section on "The Growth of the Renal Apparatus and of the Changes occurring in it," illustrated, as it is, by reference to comparative physiology, throws great light on the formation of connective tissue, and on the reason of its increase as age advances.

Holding these opinions respecting connective tissue and its *rôle* in disease, Dr. Beale is placed in opposition to the many modern pathologists who describe granular contracted kidney, and its analogue in the liver—cirrhosis, to be dependent on a parenchymatous inflammation pervading the glands by the medium of the interstitial tissue. The connective tissue described as present in abnormal quantity and position in contracted kidneys "is really composed principally of altered vessels and wasted secreting structure" (p. 23). This opinion he reiterates at p. 52, and there adds that the contraction and wasting of glandular texture may occur at an early period of development, when scarcely a trace of connective tissue can be discerned, and, consequently, when such wasting cannot be attributed to a tissue not in existence at the commencement of the morbid process inducing it.

> "The morbid changes may originate in the cells themselves primarily, or the cells may be affected secondarily through altered blood, or by an irregular, insufficient, or too abundant supply of blood. Morbid changes may first appear in the vessels; and there is reason to think that some renal diseases are of nervous origin. It is quite certain that some forms of renal disease originate at a very early period of the development of the gland, even during intra-uterine life. Some of these may, perhaps, be due to defective nutrition; while others are 'developmental' in their origin, being due to some defect in the changes which succeed one another during the evolution of the elements of gland-structure."—Pp. 511, 52.

The supply and mode of distribution of the nerves of the kidney has received much elucidation from the author's minute researches. The nerves, derived chiefly from the sympathetic, have their fibres connected with numerous ganglia and ganglion-cells, and are distributed, not only to the renal blood-vessels, to both the intertubular and the Malpighian capillaries, but also to the uriniferous tubes. The nerves supplying these tubes may be regarded as constituting an afferent system, capable of influencing, through the nerve centre, the efferent fibres distributed to the arteries. Thus they govern the calibre of the arteries, and necessarily regulate the proportion of blood flowing through the capillary vessels. The ganglia referred to are, without doubt, special nerve-ganglia of the kidney, and bear the same relations to this organ that the little ganglia, in connection with the cardiac nerves, bear to the heart.

The development of the Malpighian bodies is another physiological problem to which Dr. Beale has devoted much attention. His conviction is—

> "That a fully formed vessel does not approach the blind extremity of a developing tube, and penetrate into its interior, pushing before it the wall of the tube; nor, on the other hand, does the terminal extremity of the uriniferous tube approach and ultimately enclose the vascular tuft; but the blind end of the tube and the capillary vessels, which are ultimately found in its interior, are developed and grow *pari passu*."—P. 29.

This conviction is based on direct observation, carried out most successfully in the kidney of the newt; and it is worthy of remark that, in this animal, Dr. Beale has portrayed a short blind tube or diverticulum attached to each uriniferous tube, which he surmises may become occupied with a Malpighian body in the progress of development, and at length assume the place and function of the perfect tube adjoining, when this latter has completed its functional existence and wasted.

The notion that the Malpighian bodies are covered with an epithelium which serves to separate the solid matters of the urine, and that the watery portion is derived from the straight uriniferous tubes, is entirely discarded, as opposed to the results of direct research and of arguments deducible from both human and comparative physiology.

The mode of operation of diuretics and the rationale of deranged action are treated of by the author; but, as our space forbids a notice of both those subjects, we must content ourselves with quoting a few observations on some forms of morbid action, in the explanation of which the hypothesis of germinal matter is significantly resorted to. In all cases of inflammation there is an increase of the cellular elements, or rather of the masses of germinal matter of a tissue; and in the instance of organs like the liver and kidneys, not only is there this sort of increase, but there also occurs a passage through the walls of the blood vessels of materials derived from the blood, including much living germinal matter, which become mixed with the glandular elements, and accumulate between the tubes and capillaries. Moreover, the germinal matter, being freely supplied with nutrient pabulum, increases and multiplies rapidly, and seriously interferes with the action of all neighbouring textures. Pathologists have, for the most part, assumed the fact of the accumulation of epithelium as the starting-point of their inquiry relative to the phenomena of congestion and inflammation; but Dr. Beale proceeds a stage further in the inquiry, and seeks to explain why such accumulation takes place, or, in other words, "the circumstances under which the formation of the very permeable substances which accumulate in the blood, and which form the ordinary pabulum of the cells concerned in excretion occurs." Now, the first circumstance in the chain of causation is, in the author's opinion, an altered state of blood affecting the nutrition and action of the nerve-structures, and so giving rise to those nervous phenomena expressed by chilliness, a feeling of malaise, and shivering.

"Perhaps insufficient oxidation lies at the root of the matter, and materials resulting from disintegration in an imperfectly oxidized state may be the substances which, perhaps, under the form of 'extractive matters,' permeate the capillary walls, and supply nourishment to the germinal matter of the blood and tissues, instead of being fully oxidized and very soon removed in the form of carbonic acid, urea, &c. If such materials accumulate unduly in the blood, the capillary circulation will everywhere be retarded, although to the greatest extent in those organs which are concerned in elimination."

Hence dilatation of capillaries, arrested depuration, and, if accumulation goes on, eventually stagnation and rupture.

Yet that something more than increased pressure on the capillary walls subsists is rendered probable by the fact that the albumen in the urine often differs chemically from that present in the blood, and has, moreover, now and then been found to possess very peculiar chemical properties. The inference therefrom is that albumen of a particular kind may be actually *formed* in the kidney. Lastly, according to the author's views, the albumen in the urine percolates directly through the Malpighian vessels under the influence of increased pressure; whilst in other cases it seems to be formed by the epithelial cells, or rather by their germinal matter, as in that peculiar modification of this substance of which casts consist.

Recently pathologists have largely concurred in defining the morbid states of the kidneys associated with albuminuria, as comprised under the three conditions of tubal, intertubal, and vascular disease—a division illustrated and enforced by Dr. Dickinson, in his lately published work on Bright's disease. But, as already indicated, an intertubal morbid process, seated in an interstitial connective tissue, finds no place in Dr. Beale's category of renal lesions; and, indeed, disease originating in the blood-vessels is not admitted to be an independent condition. The lesions recognised are—the contracted and wasted kidney, the enlarged kidney, the fatty enlarged kidney, the fatty contracting kidney, the amyloïd or waxy kidney, and renal alterations connected with cancer, tubercle, and cysts. In this list it will be perceived there are two forms of fatty kidney enumerated as distinct pathological conditions. Herein Dr. Beale revives the views of Johnson and others, and places himself in opposition to Dr. Dickinson, who has taken much pains to prove that the presence of fat in renal lesions has no special pathological significance, but may be an attribute of any one among them, dependent rather upon chronicity than any other circumstance.

Enlargement of the kidney, again, is treated of as a substantive lesion, and not only as a primary stage of some further morbid action. Simple congestion may be a cause of enlargement operating temporarily, or congestion may become so aggravated that exudation follows, and then the increased size "is due, 1, to vascular distension; 2, to multiplication of cells in the uriniferous tubes; and, 3, to the formation and growth of a new material, which is formed external to the capillaries in the seat of the supposed matrix." Hence the increased weight and size of inflamed kidneys. As a rule, these phenomena give place to others, which end in producing the contracted kidney. The damaged secreting function of the tubes and cells and vessels involves a thickening of the vascular walls, shrinking and wasting of the capillaries, and wasting of the previously enlarged secreting cells. There are, however, forms of chronic enlargement, proceeding during months and years, in which the cells are much increased in number, though often reduced in size, and the tubes in places appear completely choked with them. Such increase is incompatible with the discharge of normal function.

> "In enlargement of the kidney [remarks Dr. Beale], I have seen numerous diverticula growing from the tubes at short intervals amongst the tubes already existing, not only discharging no office themselves, but interfering with the action of adjacent secreting structure which yet remains capable of activity...... Such diverticula may become detached from the tube from which they were offsets, and still continuing to increase in consequence of the growth and multiplication of the masses of germinal matter within them, at length become small cysts. This is probably one way in which cysts of the kidney originate."—P. 56.

The other modes in which cysts originate are, according to the author, by the bulging of tubes above an obstruction, and further by the wasting of a circumscribed portion of renal structure and the formation of a cavity. At the same time, he regards the pathology of cysts as yet undetermined; for he cannot accept the hypothesis of Mr. J. Simon, of the development of such structures to any considerable extent by the growth of a single cell.

As already noted, the writer of this treatise holds that the large white fatty kidney exhibiting stellate veins on the surface is a distinct lesion—

> "In no way related to other forms of renal disease. It seems to be connected with that habit of body which is associated with the development of tubercle. It may be traced, at least in some instances, to changes occurring in the kidney at a very early period of life. This is a form of chronic renal disease which is not uncommon in young people, and it is frequently met with in children."

Though differing from several pathologists in the matter of the existence and relations of the large fatty kidney as a substantive lesion, Dr. Beale is of the number of those who deny its being an early stage of the contracted kidney, a doctrine especially upheld by some foreign physicians. Moreover, he goes farther than most in acknowledging a distinct variety in "the fatty contracting kidney," in the production of which the deposit of fatty matter, the shrinking and wasting, proceed simultaneously. The following is the sketch given of the symptoms presented by those suffering from this lesion :

"I have known cases of this condition occurring in young healthy looking men, who, although they would not be called intemperate, had, nevertheless, lived far too freely during several years. The attack may come on as one of slight acute dropsy, the urine containing a large quantity of albumen and being of fair specific gravity, about 1020 or higher. There is usually little œdema, and for some time the case appears likely to progress favorably. But the albumen does not diminish. In some instances vomiting and head symptoms come on after two or three months, and the patient soon dies. The symptoms may not have been at all urgent, and up to within a week or two of the patient's death there may have been nothing in his appearance to lead us to suspect the extremely fatal form of disease from which he was suffering. Even a careful examination of the urine does not always enable us to discover the nature of the disease. . . . In some rapidly fatal cases of this form of renal disease I have noticed that the heart's action was feeble, and, at the post-mortem examination, the heart was found to be much smaller than natural. . . . I have seen cases in which nausea and headache were the only symptoms complained of."—P. 62.

A case is detailed by way of illustrating the symptoms, course, and post-mortem appearances in this variety of kidney disease, and the practical corollary is deduced that, as such cases are so obscure often, it is desirable, when called upon to examine individuals for life insurance, to investigate the chemical and microscopical character of the urine. Dr. Beale has very fully detailed the minute anatomy of this form of diseased kidney, which, as he remarks—

"Is doubtless the one which has led Frerichs and others to conclude that the large white kidney is but an early stage of the small contracted kidney; and the view is, perhaps, correct as regards some cases of this particular form of enlarged kidney, but it has no wider or more general application. This disease is quite distinct from the large, pale, fatty kidney of scrofulous persons."—P. 66.

Amyloid, waxy, or albuminous degeneration, the depurative disease of Dickinson, is described with much minuteness. It is curious to notice the diversity of statement that may arise between observers respecting simple matters apparently determinable readily enough by any one possessing eyes. For instance, Dr. Dickinson speaks of the transparent material occupying the renal tubes as presenting the characteristic reaction with iodine, whereby the existence of the so-called amyloïd substance may be predicated in any tissue or organ; whereas, on the contrary, Drs. Beale and Grainger Stewart find no such constancy in applying this test. So, again, in the matter of the solubility of "amyloïd" substance in hydrochloric acid, Kühne and Roudneff, on the one hand, assert its insolubility in that acid, whilst Dickinson affirms the contrary. Such discrepancies must, as Dr. Beale suggests, arise from want of concurrence among observers as to what is to be regarded as amyloïd, and he proceeds to take exception to some of Dr. Dickinson's recorded illustrations of "depurative" disease, and, in general, objects altogether positively to his hypothesis of the production of such disease. Indeed, Dr. Beale does not allow himself to be carried away by attractive chemical theories of disease; on the contrary, we find him protesting against the brilliant hypotheses of Dr. Bence Jones, and likewise against the ingenious views of Dr. Dickinson respecting the nature of amyloid. He is satisfied that neither long-continued purulent discharge nor the withdrawal of alkalies from the system explains the production of amyloïd; but, to his mind—

> "It seems more reasonable to consider the waxy disease, the purulent discharge, the disease of bone, or tubercle, where it exists, as the result of some cause or condition common to them all, than to look upon the first as the consequence of one or other of the latter morbid states. Much stronger evidence than has yet been advanced need be adduced to justify the conclusion that any disease results from the withdrawal of so much potash and soda from the system, and that the amyloïd deposit is of the nature of a residuum. . . . . These purely chemical theories of great pathological changes are generally very disappointing."—P. 70.

In waxy kidney the Malpighian capillaries are the first to undergo thickening. This thickening of the capillaries and of the minute arteries is not a peculiarity of "amyloïd" degeneration, but prevails also in many chronic renal diseases. Dr. G. Johnson has termed the process hypertrophy; but Dr. Beale justly repudiates this term as improperly applied, inasmuch as the increased thickening is not due to a simple addition of so much homologous tissue, as in true hypertrophy, but to a deposit differing in nature from the material of the arterial coats, both in microscopical and chemical affinities.

Cancer and tubercle of the kidney are rather summarily dealt with in about a page, and the first part of the work is brought to a conclusion by a short chapter setting forth the general principles of treatment of renal disease.

As we have made it our object in this review chiefly to set forth the views of the author regarding renal pathology, we shall pass over the next section of the work, "on healthy urine and its examination," and take up certain subjects treated in the third part, which is devoted to the "urine in disease and its examination."

In the opening remarks of his essay upon morbid states of the urine Dr. Beale makes an onslaught on the use of the word diathesis, employed in connection with some of those states. We cannot fully appreciate the objections urged by him against such use, nor are we prepared to go with him in surrendering it altogether. Such expressions as the "uric acid diathesis" and the "oxalic acid diathesis" certainly express but half truths, and are misapplied and mischievous when assumed to represent ultimate and entire pathological facts. Nevertheless, in practical medicine, they may be usefully employed as collective terms, conveying to the mind a collection of morbid phenomena distinguished by some one prevailing condition of the urine.

Dr. Beale proceeds to consider the urine as altered by a deficiency or redundancy of its normal constituents, or by the superaddition of some abnormal material. He has collected cases and given full analyses of the urine of diabetes insipidus, a disorder of more serious moment than generally supposed. When noticing urea in excess he takes occasion to combat the chemical theory of fever as a peroxidizing process, and in so doing exhibits, as a chemist, much disgust at the misapplication or misinterpretation of his favorite science.

> "The large proportion of urea excreted in fevers and inflammations," he writes, "has been supposed by some authorities to be proof of excessive oxidation, and necessarily connected with the high temperature of the body; and in spite of the blood being stagnant in the vessels, and the lungs choked up, this ill-considered theory has been so forced upon us that it will probably be some time before any view more in accordance with well-known facts will be accepted. It need scarcely be said that a rise in temperature may be brought about in many ways besides oxidation, while it may be regarded as certain that the increased heat in fever and inflammation, and the increased quantity of urea formed, do not depend upon peroxidation. There is usually a very large excretion of urea in diseases in which the activity of the oxidizing processes is much reduced." (Pp. 186, 187.)

The subject of excess of the organic constituents of the urine is illustrated by several analyses in cases of skin disease and of chorea. The absence of chloride of sodium in the urine of pneumonia, on the occurrence of hepatization, is insisted upon as a general fact; and the opinion is advanced, that during this stage of inflammation of the lungs, there is a determination of this salt towards the suffering organs, which ceases when resolution sets in.

The direct relation between the wear and tear of nerve and brain tissue, and an excess of phosphates in the urine, is, in Dr. Beale's opinion, not so well established a fact as is generally assumed. He believes himself prepared to prove "that, in this matter, speculation has, to a great extent, taken the place of reasoning founded upon facts and experimental observations." With the view of making good his statement, the author enters largely on this question of the excess of phosphates in the urine, and adduces numerous analyses made by Dr. Bence Jones, Dr. Adamson, and himself, of the secretion in various diseases and injuries of the brain, and in insanity, which, apart from the point at issue, will be examined with interest. The upshot, however, is, that the chemical examinations of the urine hitherto made are inadequate to establish the supposed relation between urinary phosphates and the wear of nerve-tissue.

The presence of abnormal soluble matters in the urine is copiously treated of, particularly in chemical details. The instructions given for testing, especially for sugar, and for estimating the proportion of sugar in diabetic urine, furnish a complete manual of reference. In discussing the nature of diabetes, the author adopts the views of Bernard on the amyloid or glucogenic matter being formed in the liver, and objects to Dr. Pavy's experiments as inconclusive. This problem of the source and mode of production of sugar in the economy is very largely considered from a physiological and anatomical point of view. According to Dr. Beale's judgment,—

> "The evidence is at present strongly in favour of Bernard's view, that, in health, sugar is produced in the liver, and destroyed whilst it is in the blood. In the normal state the destruction of the sugar occurs at the same rate as its formation, while in certain lesions of the nervous system, and under other circumstances, more sugar is formed than can be destroyed, or the quantity formed remaining the same, the normal conditions under which its decomposition takes place being absent or modified, it accumulates in the blood, and is excreted by the kidneys and other secreting organs, thus producing diabetes. . . . There can be no doubt that certain parts of the nervous system are seriously implicated in all cases, but whether the nerves exert a direct influence upon the sugar-forming or sugar-destroying process, or only affect these operations indirectly through the control they exert upon the calibre of the arteries, and therefore upon the quantity of arterial blood distributed to the capillaries, is not known; but there are, I think, many facts which favour the latter view, while it has never been shown that nerves exert

any *direct* influence upon the growth or action of any cells whatever."

Part IV. "The microscopical examination of the urine, of the various urinary deposits, of calculous disorders," is characterised by the completeness of the directions and of the details of observation furnished, and more especially still by the profusion of illustrations appended, drawn with the greatest care and accuracy, according to a specific scale.

Among other subjects comprised in this part is the peculiar condition known as chylous urine, of which the account is very complete, and accompanied with analyses. An equally complete history is given of casts of uriniferous tubes considered as urinary deposits. An endeavour is made to establish criteria for judging of the dimensions and condition of the urinary tubes by the size and appearances of the casts expelled in the urine; and Dr. Beale believes that any unprejudiced person may, by clinical study, in a short time convince himself that the nature of renal disease may be diagnosed in many cases by the microscopical characters of the urinary deposit.

In bringing this review to a close, we cannot omit referring to the encyclopædic character of Dr. Beale's book, as a treatise on kidney diseases and the urine. Its leading feature, however, is the completeness of its information in all matters relative to the urine in health and in disease. In the preceding analytical sketch we have altogether passed over Part II of the work, "Of Healthy Urine and its Examination," occupying eighty-five pages, in which the chemical history of the secretion is fully examined, and the modes of analysis, and the apparatus required for conducting it, are described at large. This chemical part of the work has, the preface informs the reader, been revised by Prof. Bloxam; the section, also, on volumetric analysis has been rewritten by Mr. Sutton.

Another feature, noticed also at the commencement of this article, is the extent of illustration by a multitude of plates, whereby a much more truthful conception can be arrived at than by any amount of descriptive text. Dr. Beale rightly considers no microscopic appearance to be so simple as to be unworthy of representation. Thus, for instance, he shows, and very accurately, the appearance of air-bubbles under the microscope, and also gives drawings of extraneous matters, such as particles of hair, of woollen or linen tissue, of feathers, starch, &c., which are perplexing ofttimes to the tyro in microscopy. Such plates will, indeed, save a world of trouble both to learners and to the learned in the microscope. The former will have numerous mysteries at once cleared up, and the latter will be saved the trouble and often the annoyance of having to explain specimens sent them by the inexperienced in microscopic work, and of convincing them that they have not made some marvellous addition to the knowledge of minute structures.

Another portion of the book that will be appreciated both by student and practitioner are the tables for the qualitative examination of urine, which indicate the changes and deposits in the urine to be looked for, the tests to be used, and the effects to follow on their application. In fine, the student, diligent in "reading up the urine," will find this treatise invaluable, whilst to the practitioner it will form a complete book of reference for information touching the minute anatomy of the kidney in health and disease, and the normal and abnormal conditions of the urine, with the best means of testing and examining them.

Review VIII.

Twenty-second Report of the Commissioners in Lunacy to the Lord Chancellor. June, 1868. Pp. 269.

Tenth Annual Report of the General Board of Commissioners in Lunacy for Scotland. 1868. Pp. 317.

These annual returns of the state of lunacy and of the condition of the insane in Great Britain will always demand consideration; but at the present time, when the conviction forces itself upon the public that the number of lunatics augments with terrible rapidity, and that the constantly extended and extending provision made for their care and maintenance is yearly pronounced inadequate, such returns possess an unusual interest.

As in past years, so now, the report of the English is drawn up on a different plan from that adopted by the Scottish Commissioners, and hence difficulties arise in endeavouring to establish comparative facts in regard to the lunacy and lunatics in the two countries. The former body pursue their well-worked—we might say with much justice well-worn—plan of publishing the information they have to give. The attempts to portray the whole state of lunacy in England for the past year, to deal with the great questions that have arisen and press forcibly for answer, and to afford the advice that a well-instructed Commission might be expected to give, are, to say the least of them, feeble.

The greater portion of the report is occupied with remarks abstracted from the entries made in the visitors' books during the year, mostly in the case of private institutions, and, as these extracts chiefly refer to minor details they are of little public interest or value. For though such particulars may be rightly anticipated from a body of asylum visitors, in evidence of their supervision and assiduity in duty, yet in their general report we might fairly look rather for general deductions and weighty practical lessons and suggestions touching the whole question of lunacy—its prevalence, the causes of its accumulation, the circumstances affecting its distribution, the advantages and defects of the prevailing asylum system, the best means of meeting future requirements, and such like important moot points open to discussion; and, what is more, such points as no single observer and no other collection of individuals in the realm are nearly so capable, by knowledge and experience, and by the possession of numberless official facts, of discussing. It is, however, but just to add that some important general topics are introduced in the report; these presently will be duly noted.

The progress of asylum building is the first subject noticed in the report. By adding up the list given in the appendix we find that, in 1867, there were forty-eight county and borough asylums, fifteen registered hospitals for the insane, and three asylums supported by the State for the army, navy, and for criminals. New county asylums were in progress of erection in Berkshire, Cheshire, Hereford, York East Riding, and York West Riding, and for the Isle of Man. New borough asylums were building for Leicester and Ipswich, and the enlargement of existing asylums agreed to for Bucks, Durham, and Northumberland. A proposed addition to the Rainhill (Lancashire) Asylum for 250 was negatived. The existing accommodation for the lunatics of the last-named county, and for those of Yorkshire and Middlesex, was adverted to by the Commissioners in their previous report as very deficient. Yorkshire has since taken steps to meet the deficiency, but "nothing whatever has been done in Lancashire or in Middlesex."

In Lancashire the magistrates are rebellious; they would substitute workhouse lunatic wards for a new asylum; but, say the Commissioners—

> "The Lunatic Asylums Act (1853) cannot be worked by such means; workhouse wards are properly available only for the reception of the harmless and weakminded; even these in such places are deprived of what the Legislature intended them to have; and as to diet and other necessaries of their condition, they are left to the discretion of variable and often changing boards of guardians. Effect can only be given to the Act of 1853 by means of such separate buildings, superintendence, and visitation, as were contemplated by the several clauses and

provisions contained in it; and the only just and reasonable course open to committees of visitors, who desire to relieve the ratepayers without sacrifice of the lunatic pauper, is to consider whether the experience obtained in the working of that statute might not now be most profitably used in devising buildings of an intermediate character between the asylum and the workhouse, supplementary, in every case, to the asylums already existing, of which they would respectively form portions, having their own officers, but otherwise under management and visitation by the existing committees of magistrates, and affording a kind of accommodation by comparison much less expensive than at present, and really more suitable to large numbers of the population of every asylum." (P. 85)

The Commissioners proceed to observe that it is the costliness of asylums that constitutes the real grievance to the ratepayers; that the class of lunatics proposed to be removed to workhouses are as much entitled to the protection of the lunacy laws as other insane; and that the accumulation of such inmates in asylums has led to the exclusion of recent cases, and to the continuous demand for more asylum accommodation.

On their part the Middlesex magistrates urge the postponement of further asylum building until the effects of the opening of the two district asylums, to be erected under the Metropolitan Poor Act, are known. That is, they propose to leave their insane poor unprovided for for two more years, the time required to complete the projected new establishments. They, moreover, reckon, from false premises, as the (Commissioners point out), upon getting relieved from the burden legally attaching to them, since the Poor-law Board contemplates providing only for such imbecile and harmless cases as may now be properly retained in a workhouse—a class which, by the reports of the medical officers, are scarcely to be found within the Middlesex asylums. The Commissioners conclude by showing the large number of mentally unsound paupers in Middlesex distributed in private asylums and in workhouses—a state of things, moreover, not peculiar to Middlesex. There is a prevailing tendency to transfer harmless chronic cases to workhouse wards, and to erect special wards for them. This is not only a retrograde step, with regard to such cases, as a withdrawing them from the protection of the lunacy laws, and placing them in irresponsible hands; but it moreover encourages the detention in such wards of recent cases with vain hopes of ensuring their recovery, and of saving the cost of asylum treatment.

The Commissioners rightly set their faces against this mode of dealing with recent cases, and describe it as at variance with the intention of the Lunacy Acts, a cruel hardship to the unfortunate patients, and commercially a blunder, inasmuch as the result is to create permanent charges by the production of incurables; whereas, under proper treatment, recovery would ensue, and a temporary cost only be incurred.

To establish their case against workhouse wards as receptacles for incurables, the Commissioners put on record several painful instances of serious ill-treatment and neglect that have occurred to such patients during the past year in various workhouses of the country. It is, however, no more than fair, in noticing such disasters in workhouse wards, to remark on the long list of serious casualties that have happened during the same period in asylums. That accidents will happen is a well-established fact; nevertheless, after perusing the details of those that have taken place in various asylums, the reader will be impressed with the conviction that not a few such were preventible, and further with the suspicion that there is something not perfect or satisfactory in the internal economy of our large, overgrown asylums, with many attendants, but very scant medical superintendence.

Another subject briefly adverted to in this Report is the establishment of special institutions for idiots Neither lunatic asylums nor the wards of workhouses are fitting abodes for those afflicted beings, and, on the other hand, their distribution in cottage homes with relatives or with strangers is a plan that cannot be commended. We are glad, therefore, to find the Commissioners advocate a relaxation of the stringent provisions of the Lunacy Acts in reference to idiot asylums, so that encouragement and facilities may be afforded to their establishment in place of the obstacles now presented by law.

The concluding pages of the Report are occupied by observations on some points of law in the Poor-Law Amendment Act affecting the interests of lunatics, and by a plea for a consolidation of the lunacy laws now in force, which are verbose, obscure, and a source of confusion to those who have to refer to them. There are no less, add the Commissioners, than forty-one acts affecting the insane subjects of Her Majesty in England and Wales,—a number in itself calculated to produce confusion and lead to unprofitable litigation.

England stands forward as the realm of non-restraint; and abroad, it is common to refer to non-restraint as the English system, and to suppose it universally adopted in this country. How far is this supposition true? Are we as Englishmen honest in our representations regarding it, and in contrasting our practice with that of the continent? These questions arose in our mind in reading this Report of the English Lunacy Commissioners, and we have collected from its pages the notices of instances in which mechanical coercion has been employed during the year to which its pages refer.

The result shows a more frequent recourse to instruments of bodily restraint than we had a conception of; or, in other words, it seems to us that, neither is non-restraint so universally practised, nor is it so peculiarly an English system as our writers have induced foreigners to suppose.

In the first place, it must be stated that the report does not notify the presence or the absence of restraint in every asylum visited; but as the employment of coercion falls within the category of "statutory inquiries," to be officially and regularly made by the Commissioners during their visits, we may assume that restraint was not in use in those establishments where reference to it is altogether omitted from the report. Again, the Commissioners appear to have always specified the cause for its employment, where this has been "for surgical reasons," though possibly they may have failed to do so in one or more instances. To begin with the county asylums and lunatic hospitals; it is in these the abolition of mechanical restraint has most largely been carried out, yet in eight among them some form of instrumental restraint is reported to have been in use during the year in the case of thirty-one individuals. Its application was demanded in five instances "for surgical reasons." In fifteen others it was limited to wearing restraint "gloves," and thirteen of these occurred in one establishment where such means are employed for male patients to prevent destructiveness and for "surgical purposes." Of the remaining number, the reasons assigned for its use are: "determined suicidal propensities," in a female; "to prevent strangulation" in one case; and, in six others in the same institution, "on account of violent or destructive propensities, or to guard against the patients denuding or burning themselves." In two instances, lastly, the reason is not stated.

The duration of its application varied much. One female was restrained a fortnight on account of a fractured clavicle. Gloves were worn by male patients for periods varying from ten days to six months; and restraint was used in the case of one woman on one occasion, and in that of two other women daily, each for the space of a week. Particulars of the duration of coercion are not given in the other examples put on record.

The Broadmoor Criminal Lunatic Asylum is not included in the analysis on account of its exceptional character. There four males wore strait waistcoats or camisoles for very lengthened periods, and one male for only a short time.

In two county asylums, moreover, the instruments of restraint had been laid aside not long previously to the Commissiioners visit, and in several among them locked strong dresses are still in fashion.

To turn to private institutions for the insane. Those in the Metropolitan district stand on a par with county asylums in the disuse of restraint. The largest among them offer no example of recourse to it. Of the smaller some enumerated as asylums are simply private houses with from two to six selected inmates, who are imbeciles or idiots; in such restraint finds no place; and even in many of somewhat larger population, only inmates of tractable habits are received. Others, lastly, are places for the reception of recent as well as chronic cases, and it is among these that examples of restraint occur, either on account of suicidal propensities or of maniacal paroxysms. In twenty private Metropolitan licensed houses with a population of 1662 lunatics the existence or absence of restraint is noted; and in only four among them did its employment take place, and this was limited to six individuals.

Among provincial licensed houses a more free use of restraint obtains. Like the Metropolitan private asylums they differ in size and in reference to the class of cases they receive. However, with two exceptions, there are none that contain above 100 patients, and, consequently, the proportion of large to small asylums is relatively less than in the Metropolitan district. In forty-eight of them the Commissioners have referred to the use or absence of restraint. The population among them was 1848. One far exceeded all the rest in having 485 inmates, and in this one, as in the case of the large establishments in the neighbourhood of London, no restraint was found. In 18 of the remaining 47, coercion had been more or less resorted to in the case of 59 individuals. Above half of these cases were distributed between two houses. Such is the history of mechanical restraint in English asylums during one year, which may, at least, be assumed an average one. Indeed, the remarks made by the Commissioners indicate that the use of instrumental coercion is progressively declining. Its comparatively free adoption in some asylums, and its total disuse in others, intimates that its employment depends on the personal qualifications and opinions of superintendents; and, in some measure, on the fitness of the building, and of its organisation as an instrument of treatment.

When foreign asylum physicians hear the praises of non-restraint, it is their wont to say that we English substitute in place of some mechanical fastenings about the body, a universal restraint of the whole man by shutting him up in a solitary cell; in other words, that to boast of success in abolishing mechanical coercion, we pursue a more objectionable plan and abuse seclusion. The best way of replying to such objections is to set forth what is the actual practice pursued in this matter. To inquire relatively to the amount of seclusion resorted to in asylums and as to its proper registration is one of the statutory duties of the Commissioners at their visits. And in their report of the state of the several asylums, they have noted in most cases the number of seclusions, and, in many instances, the duration also.

As a rule, it appears that female patients are secluded more frequently, and in larger proportionatenumbers, than males; but in some institutions the contrary obtains. In one county asylum it is distinctly stated that no instance of seclusion had occurred during the year; but few other establishments made a near approach to such serenity. It appears to be the rule that the larger the asylums the greater is the extent of seclusion; which is much as to say, the less is the supervision and the less is the presence of the medical superintendent looked for and felt. At the same time, the personal qualities of the superintendent as such count for much, as is sufficiently evident from the returns of seclusion of asylums, having nearly equal population. For example, there are three county asylums, containing respectively 475, 529, and 617 inmates, and in which severally, 87, 79, and 72 were placed in seclusion in the course of the year. There are other three asylums having respectively, 494, 538, and 690 inmates, in which the seclusion amounted in each severally to 3, 9, and 19.

Doubtless a partial explanation of such diversity in practically dealing with the insane may be frequently found in the superior arrangements and structural fitness of some asylums over others, in the relative number of recent cases admitted, and also in the varying degree of crowding of the inmates. But such an explanation is very inadequate to account for the wide discrepancy observed. To take the case of the two monster Middlesex asylums—Colney Hatch with 2039, and Hanwell with 1728 lunatics. The former would certainly claim a superiority over the latter in structural fitness. As to the character of the cases received, they are drawn from the metropolis, and may be assumed to be generally alike; yet in Colney Hatch 241 patients were secluded, and in Hanwell only 60. Three years ago as many as 500 of the two sexes together were so treated in the year at Colney Hatch, and the present reduction is entirely among the male patients. Referring to the number of occasions on which seclusion has been practised, the difference is as remarkable, being 970 in Colney Hatch, and only 119 in Hanwell.

From an analysis of the Commissioners' statements, it appears that during the year 1867, to which their report applies, the number of persons subjected to seclusion in thirty-eight county and borough asylums was 1525, and that the population of those asylums amounted to 21,513. Hence it appears that, omitting decimals, 7 per cent. of the inmates of those institutions were once, twice, or more, and for variable periods of time, put in seclusion. As already made apparent by previous calculations and quotations, the proportion of seclusions varies extremely in different asylums; but, by taking one with another, in the way we have done, it enables us to present a general result, representing sufficiently near the amount of seclusion practised in English asylums, in relation to the numbers under treatment in them.

It frequently happens that the same individual is secluded several times. Reviewing the figures on this point furnished by the report, it may be stated, approximately on the whole number of seclusions, that, on the average, each act of seclusion is repeated in the person of the same individual between three and four times.

The period over which seclusion has been prolonged in the several instances enumerated is only now and then casually referred to, and no general conclusion can be arrived at. It is, nevertheless, to be gathered that in the majority of cases it has not lasted beyond one, two, or three hours. In many instances it has been continued for only half an hour; but in others, again, few in number, to the whole day.

There is one minor circumstance to be remarked upon, and that is the occasional practice, in some asylums, of locking patients in their own rooms, without recording the fact in the register of seclusions. This neglect has necessarily called forth the animadversions of the Commissioners. The deed itself is frequently, we apprehend, an unauthorised act on the part of attendants, who, whilst being wishful to escape the trouble and annoyance of patients, and at the same time unwilling to multiply decided acts of seclusion, of which a report is required, have recourse to the expedient in question, whereby the good opinion of the superintendent as to their management of the wards, is not lessened.

Altogether English asylums, public and private, pass through the visitation ordeal of the Commissioners very satisfactorily. More exercise and employment out of doors, more walks and excursions beyond the asylum precincts; and indoors, improved means of diversion, are the points chiefly insisted upon by the Commissioners, in their recommendations to superintendents and proprietors. They have deemed it necessary, in consequence of the number of cases of ill-treatment of patients by attendants, to issue a circular to the governing bodies and proprietors of asylums, calling upon them to take proceedings against attendants so offending, instead of resting satisfied with their dismissal. With respect to the provision for religious worship in public asylums, the Commissioners now exhibit a strong and we think a commendable bias to the erection of chapels at some distance from the main building, so that these structures may constitute pleasing objects from the windows. This is an innovation upon former recommendations; in as much as for years since it has been the established principle to connect the chapel with the wards in as central position as possible, so as to render it available for the use of the patients in all weathers without exposure, and more particularly for the weak and aged.

Another structural requisite now insisted upon is the immediate connection of the apartments of the medical superintendent with the main building, and detached residences are condemned as improper. This is a matter which has two sides to it, and much may be said on the side opposed to the official dictum.

A further subject, which has latterly attracted more attention, is, the baths and mode of bathing pursued in Asylums. And assuredly, in respect to these matters much improvement is called for. In not a few establishments, two, three, four, and even six patients, have been bathed in the same water; here and there, two together have been placed in a bath. There is a prevalent deficiency in the number of baths, and the same obtains with respect to lavatories.

One of the most important appendices is that exhibiting the list of workhouses visited by the Commissioners during the year 1867. It contains only a moiety of such establishments, but it challenges attention by reason of the vast number of insane, imbecile, and idiotic inmates found in them. These unfortunate beings are, by their position, cut off from the protection and supervision afforded to the insane in asylums; they are chiefly in the hands of irresponsible and often changing guardians; are badly housed, and deprived of most appliances and conveniences to throw a beam of sunshine upon their captivity, and, as the Commissioners show, and have in past years shown, are exposed to many injuries and grievances, and to sad neglect. Many workhouses contain, especially those attached to considerable towns, 100 and upwards insane inmates, and in the new workhouse at Manchester as many as 243 are congregated. Such receptacles are, to all intents and purposes, asylums, and should be subjected to the same conditions, requirements, and regulations, as recognised asylums. Indeed, the majority of private licensed houses, which are so sedulously supervised by the Commissioners, are inconsiderable institutions compared with the great bulk of these workhouse wards. The value of the Commissioners' visits to the workhouses in the interests of the irinsane occupants is testified by each recurring report, but yet the Board is too small to admit of even one visit per annum to half those establishments.

It is beyond doubt that the existing system of supervision is, as far, at least, as regards workhouse lunatic wards, and single patients, inefficient and inadequate, and some changes in the law relative to the seclusion of pauper lunatics, to the provision to be made for them, to the authorities to be entrusted with their care, maintenance, and oversight, are imminent. The creation in the Poor Law Board of a department for taking special cognizance of pauper lunatics, and for providing for their accommodation and management, seems impending; chiefly, in connection with the metropolitan new district asylums now in course of erection. This is an event to be deplored, as dividing duties which ought to centre entirely in the lunacy commission, and as likely to give rise to misunderstandings and conflicting views and policy. We can here do no more than allude to this important question, but trust to see it elsewhere satisfactorily considered.

Another appendix of sufficient interest is occupied with plans of several recent asylums, accompanied by details. The final appendix sets forth four additional tables for keeping statistical records in asylums, on the plan agreed to by the Committee of the Medico-Psychological Association. This is a very proper recognition of the labours bestowed by that Committee, and, as the Commissioners remark,—

> "A compilation of facts registered according to this series of tables, in all institutions for the treatment of insanity in this country, would be of the greatest utility in statistical comparison, and supply the chief requisites for a scientific application of the results of medical statistics. We trust, therefore, that the visitors and superintendents of all such establishments may, as early as practicable, introduce therein this system of medical registration." (P. 81.)

In writing this analytical review of the 'Report of the English Lunacy Commissioners,' we have so much expanded our notice of the several topics contained in it, that we find ourselves at the end of the space that can be allotted us without having at all embarked upon the examination of the "Report of the Scottish Lunacy Board," which by the extent and fulness of its information, and the value of the discussions and suggestions contained in it, merits a careful analysis. In order that it may receive meet consideration, we are therefore compelled to defer the review of it to a future number.

REVIEW IX.

1. *Pathologie Générale des Maladies de la Peau.* Par le Docteur Alphée CAZENAVE, &c. &c. Paris, 1868. Pp. 386.

The General Pathology of Diseases of the Skin. By Dr. A. CAZENAVE.

2. *On Diseases of the Skin, including the Exanthemata.* By FERDINAND HEBRA, M.D., &c. Vols. I and II. Translated for the New Sydenham Society. 1866-7.

3. *Examen Critique de la Divergence des Opinions Actuelles en Pathologie Cutanée.* Par le Docteur BAZIN, &c. &c. Paris, 1866. Pp. ix, 200.

Critical Examination of the Diversity of present Opinions regarding Cutaneous Pathology. By Dr. BAZIN.

4. *Skin Diseases: their Description, Pathology, Diagnosis, and Treatment.* With Wood Illustrations and copious Formulæ. The Second Edition. By TILBURY FOX, M.D. Lond., M.R.C.P., &c. London, 1868. Pp. 472.

5. *On the Parasitic Affections of the Skin.* By Dr. MCCALL ANDERSON. Second Edition. London, 1868. Pp. 250.

6. *Clinical Lectures on the Diseases of the Skin.* Illustrated by coloured Photographs from Life. By BALMANNO SQUIRE, M.B., F.L.S. London, 1867-8.

Also, by the same Author, *A Manual of the Diseases of the Skin.* London, 1868. Pp. 276.

7. *Journal of Cutaneous Medicine and Diseases of the Skin.* Edited by ERASMUS WILSON, F.R.S. Published Quarterly. London, Jan., 1867, *et seq.*

8. *Clinical Lecture on Unilateral Herpes.* By JONATHAN HUTCHINSON, F.R.C.S. 'Lond. Hosp. Reports,' Vol. III., p. 68 *et seq.*

Also *On Frontal Herpes.* 'Ophthal. Hosp. Rep.,' Vol. V, p. 331.

Also *Hints as to the Study of Skin Diseases.* 'London Hospital Reports,' Vol. IV, p. 145 *et seq.*

9. *On the Significance of Skin Affections in the Classification of Disease.* By T. CLIFFORD ALLBUTT, M.A., M.D. Cantab, F.L.S. 'St. George's Hospital Reports,' Vol. II, p. 187 *et seq.*

DURING the last few years the clinical study of affections of the skin has been pushed forward with much energy, and with not a little success, as is shown by the constant appearance of books and essays upon the subject, by the establishment of special classes in our larger hospitals, and by the publication of such illustrations as those of Hebra and von Bärensprung, of

Hardy and Monteja, of Balmanno Squire, and of the New Sydenham Society. A great deal that is written on dermatology at present is, of course, of little value, and is published rather for the advantage of the authors than of the public. Other volumes are published only as compilations for the assistance of teachers; but we are happy to add that a large proportion remains which is of great value, and shows that the perseverance which has done so much of late to advance other branches of medicine—such as ophthalmology—has been attended with like results in cutaneous medicine.

In our present article we have no intention, nor, indeed, have we the space at our disposal, to enter into those details of cutaneous disorders which have a narrow and special interest only. Our aim is rather to review those more general results which have been worked out or suggested by special inquiry, and to show, in the first place, how we may assist the students of skin affections to claim a higher and better position for themselves and their subject than they have hitherto done or have been allowed to do; and, in the second place, to show what newly discovered or newly verified facts there are in this branch of our work which have important bearings upon the larger questions of physiology and of medicine.

The treatises which we have cited are not chosen, therefore, with the view of presenting anything like a complete list of the really good writings on dermatology which have recently appeared, nor are they chosen at random; but we have selected those which are likely to serve us best in the present article. Hebra's great work is the best representative of the mere dermatologist. The author knows skin diseases well, discusses them admirably, takes as large a view of their pathology as his peculiar field of experience will allow of, and as useful a view of their therapeutics as can be attained by a physician who translates all the bodily functions into the language of his own especial study. We believe that the permanent value of this laborious and, in many ways, most excellent treatise, will prove to be much less than is generally supposed. Dr. Cazenave publishes a new edition of his well known and useful book, in which he discusses modern views of dermatology with much point, if not with much breadth. Dr. Bazin is a champion of a more comprehensive mode of regarding the diseases or affections of the skin. We quote a recent and characteristic publication of this physician, whose views are in decided contrast with those of Dr. Cazenave. For Dr. Tilbury Fox's book we have a strong liking; we see reason to differ from the author in many places and on many points of importance, as we shall say hereafter; but the spirit of industrious and accurate observation which marks his writings, and, moreover, the excellence of his method, command our hearty approval. Dr. Squire we have to thank chiefly for a very useful and well-executed series of photographs; and the accompanying letter-press is simple, clear, and good. Besides the shorter essays, which contain matter which will concern us, we have thought it our duty to mention the 'Journal of Cutaneous Medicine,' which has now completed its second volume. The duty is by no means a pleasant one; and were it not that the journal claims to represent English dermatology, we would gladly have passed it by on the other side. It first appeared under the joint editorship of Mr. Erasmus Wilson and Dr. Tilbury Fox; and we must presume that this arrangement is something more than nominal, though we detect throughout a great deal of Mr. Wilson and very little of Dr. Fox. Far be it from us to forget what is due to Mr. Wilson. At a time when dermatology was neglected he did a vast deal to claim for it a proper place in medicine, and to support the credit of the English school. He was never a very close thinker, nor ever a clear and simple writer; but he had a genuine enthusiasm for his subject, and gave great pains to the study of it. In the 'journal' we are sorry to see that his virtues are fading, and that his faults are growing. We cannot but trace the hand of the senior editor in a great deal of that kind of padding which makes some critics thank their stars they are not expected to read what they review, and which, in a scientific publication, to be read by earnest and busy men, is peculiarly exasperating. The perversity of Mr. Wilson's views on the subject of parasitism, again, is unfortunate, to say the least of it. At a time when microscopics are discovering parasites in every tissue and in every secretion, when their unguarded assertions do really require careful checking, and when the whole question of the development and propagation of germs this and that way is full of the most important and interesting difficulties, we look to see these difficulties treated in a very different spirit to that of the 'Journal of Cutaneous Medicine.' Not less irritating is the affectation which has perverted the customary spelling of the common names of skin affections. This is very inconvenient, and is even more vexatious to scholars than to ordinary readers. We are not by any means alone in doubting the wisdom of the change in the common spelling of Greek names by real scholars, such as Mr. Grote and Mr. Cox, when writing on very different subjects, and to drag these questionable novelties into the already ricketty nomenclature of medicine, where no such reasons exist for the change, seems to us quite undesirable.

We have said that it is neither possible nor suitable to our present purpose to enter into any complete survey of modern dermatology. We shall now enter only upon a discussion of one or two parts of the subject which seem to us to involve issues of very great importance. We purpose, in the first place, to say a few words upon the question of method, on which so much depends; how are we, that is, to co-ordinate the knowledge which we have gained concerning the various modes of morbid change in the skin; and, again, how are we to include such systematic knowledge or such results in any comprehensive view of nosology? It is a matter of great surprise to us to see how little, even yet, the medical profession is really aware of the great importance of this assimilation and construction of their knowledge.

Writers heap up a great quantity of facts, of clinical facts, and of pathological facts; and, having done so, they either present them to us in a heap as they are, or they scratch them up into lesser heaps, without any regard to their fundamental kinships, as old geologists used to classify stones; or, again, they have a minute knowledge of a certain limited branch of clinical study, and they arrange their little world as best they can in ignorance of the system of knowlege to which it is really subordinate. It is quite a melancholy fact that we can scarcely point to any one book in the English language where a serious and competent writer has attempted to deal with this great question of classification.[1] Here and there we come across a glimpse of something better; we have a passing suggestion of some larger conception, which is to combine and regulate our investigations; but not only does it escape us, as in the present state of knowledge we might expect, but we do not see that writers on nosology have any adequate notion of what is to be sought. They have never told their hopes. Meanwhile we are encouraging, on the one hand, a purposeless accumulation of details on what we are pleased to call the Baconian method—facts which we hope may serve somebody some day; while on the other hand we are failing at present to satisfy men of a more generalising temper, who are consequently indulging their tastes in phantom hypotheses and loose thinking, which may or may not fit the realities.

In asking whether diseases of the skin are to be classed apart as a happy family among themselves, or whether they are to take their chance among all other diseases, we seek to know whether their classification is to be a natural one or an artificial one; whether, that is, it is to express the order of nature or to serve some passing convenience. There is no more reason that all diseases of the skin should form a class apart than that all diseases of the abdomen, chest, or head, should be regarded as so many natural groups. The modes of their appearance in each case will depend very largely, no doubt, on such determining causes as the texture, degrees of vascularity, mechanical, and other conditions of the parts where they occur: but these things only influence the time and manner of manifestation, and must not be made, as they are constantly made, the basis of classification. They may properly determine the arrangement of subordinate groups; but the formation of classes must depend upon affinities of far wider meaning. We shall only discover these affinities by prolonged and thoughtful clinical observation, by a wide appreciation of the coexistence and succession of morbid changes in individuals and in families, by a careful comparison of all those states which we include in the term "healthy," and by a discovery of the variations which time and local peculiarities produce in the several manifestations of one morbid tendency. This is clinical science. as opposed to mere clinical record, and we earnestly hope that the Clinical Society may soon perceive and enforce the distinction.

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[1] Dr. Wilks has recently dealt with this question in the recent volume of 'Guy's Reports,' and he reminds us of Dr. Laycock's valuable efforts in the same direction.

Let us now endeavour, however feebly, to illustrate our remarks from the study before us. Let us try to apply true principles of classification to the classification of affections of the skin. We hope to show that the phenomena of these affections may be combined in three schemes of advancing complexity, and we shall take these groups in the usual order, that is, we shall begin with the simplest and end with the most complex. In proving this of skin affections, we are only applying to them a principle, which, however it may vary in its special applications, is, nevertheless, the interpretation and the key of all other diseases.

What, then, are the most simple and the most general of the phenomena which compose what we call eruptions? We have to deal with disturbed nerve-action, such as is shown by pain, itchings, numbness, gooseskin, and the like; with distorted vascularity, such as hyperæmia and anæmia; with excess or defect of colouring matter; with excess or defect of normal changes, such as sweating, sebaceous discharge, dryness, scaling of the epithelium, and the like; with abnormal changes, such as exudations into the substance of the skin, or into particular parts of the skin, or upon its surface; and with ruptures of continuity, such as ulcers (deep and superficial), fissures, scars, and the like.

Now, in our first effort at naming, we agree to name these various changes in some such way as the following:—Erythemes, Papules, Scales, Blebs, Pustules, Vesicles, Tubercles, Blotches, Ulcers, Scars.

Having made this early and simple effort to group and name, we now pass on to form and name other groups, which are of higher complexity, and include processes which combine two or more of the members of the former groups. To do this, we must carefully observe how variously, but, on the hand, how uniformly, the members of these groups combine in more complex processes, and we shall give different names to these as we observe uniform differences in the co-existence or in the succession of their combining elements. It is in this first synthesis that Willan did great service and won his lasting reputation, and it is by dissolving his work that many able writers on skin diseases now vainly hope for the advancement of their science; If Willan's constructive effort is a bad piece of architecture let us demolish it forthwith; but do not let us flatter ourselves that, by destroying the definitions of Willan and expanding ourselves in the indefinite, we are making wide generalizations, and are, therefore, philosophers. We all admit that Willan did not complete his work, and that some parts of that which he did must be remodelled; but we do also assert that the main structure of it is good and stable, and the method of it admirable and permanently right.

We have always felt deeply obliged to Dr. Tilbury Fox for his vigorous defence of Willan's doctrines in this Review and elsewhere. We shall have presently to show that Dr. Fox has yet to acknowledge the necessity for a still larger synthesis; but of that anon. Other very able writers, such as Dr. Anderson and Mr. Hutchinson, appear to be open to the charge of sapping our orthodoxy without offering us an equivalent in its place—a proceeding which is well known to be horribly immoral. They pull down the house, and they arrange all the stones in a tidy heap in one place, and all the bricks in another; the timbers they carefully collect together—for must not all pieces of wood belong to the same category—and they pile them in an elegant pyramid; the glass and metal-work, again, they place neatly each in its own classification, and when all is arranged they call their friends and their neighbours together. Now, the fault we find is not that a badly-built house is demolished, but that the authors do not seem to us to have any adequate idea of what Willan wished to do, and of what they themselves ought to do. They say, here is an eruption which a Willanist would call eczema, but, lo! here be papules; now papules are lichen, let us call this, then, lichen-eczema: or, again, lo! here be scales, let us call it eczema-psoriasis; or, while we are about it, here are both scales and papules, let us call it lichen-eczema-psoriasis—the name is a little long, perhaps, but what of that ?—it is nothing to methyl-ethyl-amylo-phenyl-ammonium. Or, if you dislike long words, we will pass the sponge over lichen and psoriasis, and call the whole lot eczema. At the same time we might expunge the word diagnosis also. Now, whether they know it or not, these able writers are surely undoing what has been done, and are going back to our first platform of scales, papules, vesicles, blebs, &c., and it is hard to see why they use any such word as eczema at all. They can scarcely suppose that Willan never saw that papules sometimes occur in eczema, though, by the way, these papules are not like those in lichen ; nor, again, that he was not aware that eczema was often scaly, though the scales, again, differ from the scales of psoriasis in being, for the most part, notshed epithelium, but dried exudations mixed with epithelium.

Willan knew this, but Willan did not mean by eczema merely an acute crop of vesicles; by lichen merely a crop of papules; by psoriasis merely a crop of scales, as some modern writers seem to think that he did; being a straight-forward man, if he had meant this he would have said so in such and in so many simple terms. Willan and his followers mean by eczema an affection composed of several elements, in which these elements preserve certain definite and tolerably constant relations to one another, and in which their order is not the order of lichen, or the order of psoriasis. Take an eczematous outbreak at one period of its course, and it may be indistinguishable, say, from some stage of psoriasis ; but mark its history throughout, learn what stages it has passed through, learn that before it was worn down into the less characteristic chronic stage it had passed through a livelier youth, and when at its height presented a crop, or crops, of fine vesicles with subsequent exudations; here you have a series differing totally from the series we name psoriasis. In this way, did our space permit, we might proceed to contrast these various anatomical series, and show how they present real differences and uniformities in spite of some apparent confusion. We are compelled, however, to proceed to the next stage of our inquiry—having classified affections of the skin among themselves, what place, if any, do these processes take in general nosology ? Are affections of the skin to be looked upon as local maladies, having an independent system of causation, owning no relationship to affections elsewhere, not to be included in family groups with affections of other organs, and not needing anything more for their interpretation than an accurate description of their anatomical peculiarities; or are they the fruits of certain constitutional qualities, having a like causation with affections of many other organs, and needing for their interpretation a knowledge, not of the skin only, its various structures and various modes of failure, but also of some or many diseases elsewhere which spring from similar causes, and ought to be grouped with them accordingly?

While we ourselves take a very distinct position in answer to this farther inquiry, yet we find that almost all writers on skin diseases either imperfectly comprehend the nature of the problem, or, comprehending it, give most uncertain replies. In this, however, they come very little behind systematic writers on other medical subjects. We find that three kinds of attempts are made to solve the question of classification; the first kind is the effort made to stretch the anatomical classification beyond its limits, and to make it bear more than it will bear—this is Hebra's method; the second kind is the effort made by writers like Hardy and Tilbury Fox, who are compelled of necessity to recognise constitutional causes, but who do so grudgingly, and, therefore, incoherently; the third kind of effort we see in Bazin and his followers, who accept constitutional causation in its full logical and practical bearings, and whose fault consists not so much in their method, which is fairly good, but in their excessive haste to squeeze all phenomena into their cadres. For brevity's sake we will illustrate our remarks from Dr. Fox's classification only. His combination is as follows:

1. Zymotic diseases (eruptions of acute specific diseases).
2. Local dermal inflammations: erythematous; catarrhal or eczematous; plastic or lichenous; suppurative or pustular; bullous.
3. Diathetic, including strumous, cancerous, syphilitic, leprous (or true leprosy), and alphous (or *Lepra vulgaris*) diseases.
4. Hypertrophic and atrophic diseases.
5. Hæmorrhagic (purpura).
6. Neurotic or nerve disorder, pruritus, prurigo, &c.
7. Chromatogenous or pigmentary alterations.
8. Parasitic, animal, or vegetable.
9. Diseases of the glands and appendages of the skin.

Now, if we look into this, we shall find that these equivalent groups, instead of being formed upon one principle, or upon two or more principles of equivalent value, are formed upon different principles used indifferently. In Class 1, we have a natural group based, not upon the anatomical characters of the eruptions (for these are ignored, or smallpox should be carried off to ecthyma, and scarlatina to Class 2 of erythemata), but upon a similitude of causation and clinical evolution. The group is a good one. But when we come to Group 2, we find a different principle: here we are guided only by anatomical characters, and we have a Linnæan method immediately contrasted with the natural method of Class 1.

Thirdly, we have a class of "diathetic diseases." Here we are not only professing to desert anatomical considerations, and to seek again a principle of arrangement in clinical relations, but we have the word *diathesis* used, as it almost always is, in a very confusing manner. Diathesis may mean either of two things, and it cannot mean both; it may mean a certain structure common to a group of individuals or families, which structure manifests itself not only in certain normal variations, such as texture of hair and skin, size and proportions of growth, peculiarities of temperament, and the like, but also in a certain tendency to tolerably constant orders of abnormal changes; all this is to be ascertained by *clinical* inquiry. Or diathesis may mean a tendency to produce certain kinds of morbid "deposit," as, for instance, cancer or tubercle, or certain kinds of failure like those in the "hæmorrhagic diathesis;" all this is to be ascertained by *anatomical* inquiry. We may take either of these meanings, and the first is the better; but having taken one of them let us use it consistently. Dr. Fox includes under "diathetic diseases," the "strumous diseases." Now, on the one hand, we have in struma many affections which are not accompanied by tubercle, while, on the other hand, tubercle occurs in, at least, one other state of body besides the strumous; so he here uses "diathesis" in the clinical sense. The next diseases, however, which he includes under this head are the "cancerous;" so that he either now leaves the clinical use of the word "diathesis" and takes the anatomical use, or he assumes that all "cancers" belong to one kind of constitution, which is improbable, to say the least of it. In Class 4 we go back again to the anatomical principle, and distinguish the hypertrophic and atrophic diseases of the skin. In Class 5 we have also the same principle. Class 6, of neurotic disorders, shows another principle, differing from any of the preceding; it is a collection of the disorders of one *system* of the body. This class ought, for instance, to include herpes zoster, in which case we have neither the principle of local anatomical appearances, nor the principle of diathesis in either of its two senses; yet nervous disorders are among the most interesting of diathetic peculiarities.

In saying all this we are far from wishing to discredit Dr. Fox's work; on the contrary, we select his classification as one of the best and simplest; the same inconsistencies are as evident, or much more evident, in other efforts of the kind. Now, it may be said, that such criticism as this is well enough as a matter of philosophy, but has little or no practical application. Nothing can be less true than such an assertion. Nothing tends so much to vitiate observation, to vitiate common language and teaching, and to muddle therapeutics as a confusion of thought and language, which is always insinuating the wrong and insensibly disguising the right. We can only state very briefly and inadequately our own suggestions for a thorough reform in our habitual way of looking at skin affections, and, indeed, at all affections whatever.

Let us take Dr. Fox's classification, and endeavour briefly to reconsider it. The reader will be good enough to remember that we have already advanced two steps; we have made a general classification of skin lesions into blebs, papules, ulcers, and so on, and, secondly, we have mainly followed Willan in his classification of these elementary lesions in groups, such as eczema, psoriasis, &c., according to certain tolerably constant variations in their observed coexistences or modes of succession. Having got thus far, we have to co-ordinate these groups again according to still wider affinities, according, that is, to their observed coexistencies or modes of succession when associated with affections of other parts or organs. The remarks we have to make on this head differ only in their application from those we should like to make concerning the affections of any other parts—of the lungs, for instance, of the kidneys, of the liver, of the brain, and so on. They are, in fact, guiding nosological principles, illustrated in the special instance of skin affections.

The first and chief distinction to be made at the outset is between two great classes of disease—those which arise from causes accidental to the individual, and those which are constitutional or natural to the individual.

The first division includes Class 1 of zymotic diseases, which is a good natural class, and under whatever name is to be retained, and Class 8, the good natural class of parasitic diseases; it includes also a part of the pigmentary alterations in Class 7; for instance, it includes ephelis, and does not include Addison's bronzing; it includes a great part, again, of Class 2; for instance, those forms of eczema which are due to contact of irritating substances, of urticaria, which are due to the ingestion of certain poisons, and so on.

The second division, of native diseases, includes some forms of all the affections in Fox's Class 2, all in Class 3, all in Class 5, if we omit the ecchymosis of bruises, and those ascertained to be due to poisons; all in Class 6, with a few unimportant exceptions, and so forth.

Thus, we obtain two great divisions, or kingdoms, of disease, which are equivalent, if not co-extensive. Can we now get a little closer to the matter than this?

There is less difficulty in subdividing the kingdom of diseases due to external causes than in dealing with the other, as their antecedents are simpler. We need not stay long to point out into what natural classes these agencies may be grouped.

The class of parasites forms a good natural group, which is again divisible into animal and vegetable parasites; the medicinal rashes make another natural group; another may consist of the eruptions which follow the application of irritants, such as liniments, the liquors used in certain trades, and so on. It is when we come to subdivide the kingdom of diseases natural to the individual, of the diseases which are the proper outcome of his own mode of growth and change, that we are met by the difficulties which belong to the most difficult branch of clinical investigation. A perfect scheme for the subdivision of this class can only be attained when our knowledge of clinical causation is perfect, for it will be the expression of that perfection.

Meanwhile we must, as a means of progress, endeavour to formulate such knowledge as we have, in order to secure our possessions, and to point the way to farther discovery.

Now, setting aside, as we have done, the great and important kingdom of diseases initiated from without and accidental to the individual, do we or do we not find among those native to the individual any uniformity of development, and any constant mutual relations? A great deal depends upon the answer we give to this question. Upon it depends this, whether we are doing all that is possible in naming a disease after its local development only, such as "eczema" or "bronchitis," or whether the occurrence of this local change has or has not any solidarity with the tendencies of the whole organism—with tendencies which may show themselves in this local disease at one time, and in other local diseases at other times. Does the human body, that is, tend to pass through certain cycles, cycles which include both healthy and morbid events, and are sufficiently uniform to admit of formulative comparison and naming; or does it always take its illnesses as casual errors which may, no doubt, affect it as a whole in proportion to their gravity, but which being originally mere local deviations from an otherwise disinterested constitution, may be described in their phenomena and causation without any reference to things beyond themselves. In a word, is "eczema," or is "bronchitis," or is "white-swelling," one expression of a general bodily tendency, a sign of a special state of the whole organism, or is it significant of nothing more than itself?

If we admit that any given skin eruption due to internal causes is not a self-consistent process, but one which the whole body conspires to produce, what do we mean by the incessant use of such expressions as "a psoriasis *modified by* a gouty constitution," as an "eczema *engrafted upon* a scrofulous habit," or, again, as "a lichen *aggravated by a neurotic diathesis*?" Why, surely these things are the "constitution," the "habit," and the "diathesis." How are these bodily states known to us except by these and such phenomena? and what can clinical observation be if it is not the sorting of morbid and peculiar phenomena in such a way as to separate the incidental from the uniform, and the grouping together of those events which seem to arise from like or the same processes of development? Can there be such a thing as an ideal bronchitis or a typical eczema apart from particular cases? Certainly there cannot. If, as it sometimes happens, two diatheses meet in one individual—the scrofulous and the syphilitic, for instance—the result in development will be a compromise of some such kind as the brood which may be got between hares and rabbits; the entanglement will be an awkward one to unravel, no doubt; but it will be unravelled the more easily the clearer our previous knowledge of the development of the parents. Having determined, then, say in eczema of internal origin, that a certain part of the skin when "irritated" in a certain way is liable to present a definite series of local changes which may thus be named, our next question must be, What other orders and disorders are there which own a like causation, and may precede, accompany, or follow such an irritation? For example, let us suppose that such an irritation of such a part of the skin may be associated with submaxillary adenitis and caseous pneumonia; with acid dyspepsia, melancholia, podagra, bronchitis, and rough kidney; or, again, with neuralgic dyspepsia and asthma, and so on. Now, caseous pneumonia is not associated with interstitial nephritis, or with asthma, so we observe an eczematous mode of change in frequent association with other disorders, which, however, are not associated with one another.

Here, then, comes in our higher synthesis—our clinical synthesis as distinguished from and added to our previously completed anatomical synthesis. In what series of morbid events are eczema, psoriasis, lichen, lupus, &c., to be included? If in more than one, in how many, and what is to be the distribution? This is the great field for cautious and discriminating clinical vigilance; the path of discovery which will richly repay us in revealing the real affinities of apparently dissimilar and distant morbid events, which will lead us to know the natural tendency of constitutions and diseases, and which will teach us the intelligent use of therapeutical means, founded upon a true insight into causation in its widest sense. We would willingly have pursued this fascinating inquiry, and endeavoured, after our poor ability, to sketch out our conceptions of what such a natural classification of diseases should be. Unfortunately we have already outstepped our limits.

We had also hoped to discuss fully another subject in this article—one which is of great interest, and which is better discussed, perhaps, in connection with skin affections than with others. We refer to the difficult subject of the dependence of nutritive processes upon nervous connections. Mr. Hutchinson has written some very interesting essays upon the question, and has in this direction again assisted in removing skin diseases from the cold shade of specialism, and vindicated their right to the careful attention of all clinical observers. Few observations could be made which have wider and more important physiological bearings than those made by Hutchinson, Eulenberg, and others, upon herpes; and it is probable that not herpes only, but many other skin affections, such as eczema in some of its kinds, are largely dependent upon nervous changes. It is better, however, for us to deal carefully with this subject at some other time than to deal inadequately with it now.

Review X.

1. *Report on the Cholera Epidemic of* 1866 *in England. Supplement to the Twenty-ninth Annual Report of the Registrar-General of Births, Deaths, and Marriages in England.* London (Printed by Her Majesty's Printers), 1868.
2. *Ninth Report of the Medical Officer of the Privy Council, with Appendix.* 1866. London, 1867.
3. *The Science and Practice of Medicine.* By William Aitken, M.D. Edin., Professor of Pathology in the Army Medical School. Fifth Edition. 1868.

Dr. W. Farr's work embraces "the chief facts and results of previous observations," a view of the topographical distribution of the disease, and many facts of interest supplied by the officers of health of towns, and provincial medical men.

The history of the cholera of 1866 is given in chronological order as originally published in the weekly reports. Two of Professor Frankland's reports, and the sanitary proceedings of the several London districts, and of those of the Registrar General's Office, are embraced in the work.

Important information on the subject of the water supply of Paris and Rome, as well as on that of London, is also given, reference to the work being rendered easy by the addition of an index.

"Asiatic cholera has visited England four times. It first appeared in October, 1831, and in that and in the following year was fatal in many parts of the kingdom. Many thousands of the people were attacked, and many thousands perished of this new disease. The numbers are unknown, as no registration of the causes of death then existed. In the United Kingdom the deaths of 52,547 persons were reported through various channels to the Board of Health.

"The disease created consternation; and although it was observed closely, and combated by the most active remedies, little progress was made in the discovery of causes."

The energy and skill, as well as the courage and judgment, of the medical profession was, during that period of political excitement and election riots, as well as of awe caused by the disease of so novel and so appalling a nature, tried to the utmost; but, happily, results such as those that occurred in Hungary, and even in Paris, where suspicions of occult poisonings drove the people to madness and murder, did not arise in the United Kingdom.

Although the 'Annual Register' (1832) fairly comments on the many points of uncertainty and ignorance in which the disease left the medical profession, there were those who did not allow the opportunity to be lost of gleaning some knowledge from the teachings of so dire an instructor.

The fact that diarrhœa preceded cholera in the great majority of cases was recorded as early as 1832.

> "It was found that the cholera in its worst form was preceded by diarrhœa, and that this diarrhœa was in some cases a mild form, in others a first stage of the disease. Now, to arrest this diarrhœa is to prevent cholera, as to extinguish a spark is to prevent a conflagration. It is true that perhaps ninety cases out of a hundred of epidemic diarrhœa left to themselves go no further, but it is equally true that the ten residual cases turn into cholera if they are not treated in the first stages, and the fact that one of the ten, even with treatment, turns into cholera, does not invalidate the practice."

Sir Thomas Watson, at King's College, delivered in his lectures, with his usual lucidity, the lesson of 1832, and discussed the qualifications of the doctrine.[1]

> "The epidemic cholera (in 1832) made its attack in two different modes. In one it seized upon the patient suddenly, and without warning; this was comparatively rare. Much more commonly the specific symptoms were preceded for some little time, even for some days perhaps, by diarrhœa; and this I take to be the most important practical fact that was ascertained during its prevalence among us. When the disease was once fairly formed medicine had very little power over it; but in the preliminary stage of diarrhœa it was easily manageable. Unfortunately people are inclined (especially those classes of the community among whom the cholera most raged) to

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[1] Medical Session, 1836–7.

regard a loose state of the bowels as salutary, and to make no complaint of it, and to do nothing for it; or in other cases they conceive it to proceed from some peccant matter within, which requires to be carried off, and they take purgative medicines to get rid of it. Both of these are serious, and often fatal, mistakes. Mere neglect of the diarrhœa, frequently permitted it to run into well-marked and uncontrollable cholera; and the employment of purgatives hastened or insured that catastrophe. The proper plan of proceeding, I am convinced, was to arrest the diarrhœa as soon as possible after its commencement by astringents, aromatics and opiates. You may object, perhaps, that the cases that were cured in this way were not cholera at all, and never would have been, but simply ordinary diarrhœa. It is impossible to *prove* the contrary, no doubt, but the presumption is strong that that the diarrhœa would, in many, and perhaps in most instances, have run on, if not checked, into the most perilous form of the disease. In many places when, taught by experience, the authorities established diarrhœa dispensaries, to which those attacked by looseness of the bowels were warned and invited to apply, that the looseness might forthwith be corrected; in many such places the cholera, which had before been cutting off the inhabitants by scores and by hundreds, began instantly to decline in frequency. I venture to advise you, supposing the disease should reappear, or whenever in the autumn a suspicion arises that this form of cholera is present in the community, not to try, in cases of diarrhœa, to carry off the presumed offending matter, but to quiet the irritation and to stop the flux as soon as you can."

The value of this discovery, established in 1849 and 1854 led to the system of house-to-house visitation. Dr. Farr believes the evidence of cases of algid cholera occurring without premonitory diarrhœa to be most imperfectly established.

In 1848-49 additional facts were ascertained, and the records of 53,293 deaths of English people by cholera, and of 18,887 by diarrhœa,

"Out of 17,564,656 living in a great variety of circumstances, are thus briefly summed up in the second paragraph of Dr. Farr's report of that period. In following cholera through its fatal way, however, the inquirer meets with some grounds of consolation. He sees places on every side which the epidemic passed over, leaving the inhabitants in the serene enjoyment of health and complete immunity. And the hope is, perhaps, not fallacious that an examination of the results of the second may be the means of mitigating if not preventing a third invasion, for whatever may be the immediate cause of cholera, it will appear evident that in England it is only seriously fatal under certain known physical conditions, which admit to a great extent of remedy."

Dr. Southwood Smith, Dr. John Sutherland, and Mr. Grainger were members of, or associated with, the Board of Health of 1848-49, &c.; they considered the cause of the malady to be veiled in the "same impenetrable mystery as ever," but not, in the common acceptation of the term, contagious, and as spreading by an atmospheric influence. Moreover, the various theories are discussed, the zymotic theory is developed, the cholera matter is named *cholerine*, and an account is given of the doctrine of diffusion by water.

The valuable Report of the College of Physicians, by Dr. Baly and Dr. Gull, appeared after the epidemic of 1849.

The visitation of 1854 found Sir Benjamin Hall President of the Board of Health, who obtained the aid of a medical council, consisting of the then President of the College of Physicians, Dr. Paris, Sir James Clark, Drs. Alderson, Arnott, Babington, Tweedie, Baly, Mr. Lawrence, Vice-President of the College of Surgeons, Mr. Simon, Mr. Richard Owen, Mr. Ward, Master of the Society of Apothecaries, Mr. Bacot, and the author of the report under consideration.

Dr. R. D. Thomson and Mr. Glaisher were appointed to conduct inquiries in the direction of chemistry and meteorology, and Dr. Hassall gave his aid with the microscope.

Valuable information was thus obtained, especially in reference to the amount of impurity existing in much of the water supplied to London. Dr. Snow's theory of the diffusion of cholera by water was confirmed in Dr. Farr's opinion.

> "Thus, by the year 1866, from observations of the three great plagues, we had learnt enough of the causation of cholera to justify us in believing that in London it could be confined within narrow limits,—in the first place, by preventing any extensive distribution of the cholera-stuff through water, as the companies, in compliance with the Water Act of 1852, had, it was believed, since 1854 carried out all their purifying filtering works; and in the second place, by the organization of health officers, who could secure attention to the early treatment of premonitory diarrhœa, and to the destruction by disinfectants of the cholera flux."

Dr. Farr refers to the appendix containing the weekly returns, in which the actual facts are given.

Several inquiries have been held before different tribunals. The Royal Commission on Water Supply "has not, as yet, reported" the result of its inquiries.

Visiting many parts of Continental Europe in 1865, cholera also carried off some persons, in the same autumn, at Portsmouth, Southampton, and Epping.

It was, however, in the second week of July, in the following year, that the disease broke out in London; and the deaths, in the five weeks that ended on 4th August, were 11, 63, 481, 1097, and 1178.

"Dividing London, including West Ham and Stratford, into two portions; in that supplied by seven water companies (Grand Junction, West Middlesex, Chelsea, Southwark, and Lambeth, from the Thames, Kent from chalk wells, New River from wells and Lea River) the deaths from cholera were, 10, 25, 61, 142, 196; but in the sub-districts, supplied wholly or partially by the East London Water Company, the deaths in the contemporaneous five weeks were, 1, 38, 420, 955, and 982.

"Thus in the first week the East London field had *one* death, the rest of London had *ten* deaths; in the fourth week the deaths were 142 in the rest of London, and in the East London field 955 deaths from cholera alone. Several of the earlier deaths by cholera in other districts happened to persons who had come or been sent from the East London field. Each death by cholera implied two attacks about two days before death. And for every attack by cholera there were about four attacks by diarrhœa, approaching more or less in character to cholera. As the epidemic rose, so it declined more rapidly in the East than in the other regions of London.

"The total deaths in London, with West Ham and Stratford, were 5973 by cholera, 3197 by diarrhœa (about 800 referable to the epidemic)—9170 by the two maladies. It will be observed in the tables that, although unquestionable cases of the disease occurred in every sub-district of London, and in some the number of deaths was not inconsiderable, the enormous disparity between the earlier ravages of the epidemic in the eastern water-field and the rest of London was never effaced."

The water theory of the transport of cholera advanced by Dr. Snow in 1849, and the statement of Dr. Richardson that the cholera-matter is an "alkaloïd organic poison, which, soluble in water, but admitting of deposit on desiccation, passes easily from one person to another under the agency of certain peculiar physical states," are with the alleged discovery of cholera fungi by Brittain, Swayne, and Budd in 1849, and the recent researches by Hallier and Pacini, quoted by Dr. Farr. Pettenkofer has shown that those places in Germany with the water-line nearest the surface suffered most from cholera. "The excretions of cholera patients give the germ, the soil developes it," says Pettenkofer. This is founded partly upon Thiersch's experiments, which have been repeated by Dr. Sanderson, who has shown that paper saturated in cholera flux and dried, when eaten produces the disease in a transmissible form in mice. The fresh flux the first day after exposure in the air is almost inert, more active on the second, at its maximum on the third, less and less active on the fourth and fifth, and inert on the sixth, day of transformation; these experiments were made at a mean temperature of 56°; a second series at a mean temperature of 49° failed.[1]

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[1] Ninth Report of Medical Officer of the Privy Council, p. 452.

Cholera matter gives water an opalescence, even though present only in very minute quantities, and after the liquid had been passed through filter paper.

Cholera matter is often yielded by persons having diarrhœa and not suffering from the characteristic symptoms of Asiatic Cholera.[1]

> "It is not improbable that *cholrine* is to some extent carried up from warm liquids by watery vapour."

Cholera flux is of low specific gravity (1008). Dr. Farr shows how the waters of contaminated reservoirs may vary in zymotic strength from hour to hour, and how, owing to the enormous multiplying powers of the cholera molecules, the waters of even such a considerable river as the Thames may become contaminated.

Pacini has found the germs of vibrions (*Molecule vibrionali*) to be of less diameter than $\frac{1}{25000}$ of an inch, and that one cubic inch might contain 15,625,000,000,000 germs. It is evident one cholera patient may disseminate many millions of millions of cholera particles in water.

The air, more voluminous than the water of a city, will proportionally contain less cholera germs; it is well, too, that the spread of the disease, judging from analogy, requires the presence of the germs in certain quantity. Dr. Farr quotes the writings and experiments of Vierordt, Newport, Kölreuter, Naudin, and Gärtner, and adds:

> "The infection power of cholera liquid is essentially transitory; it is developed in given circumstances in its intenser form, and in a community as well as in an individual,—in India as well as in England,—it grows as well as declines by a law of its own; it is epidemic only for a time, and by periods of years; it has its seed-time and its harvest in each locality; and the air or the water which on one day is poisonous may a few days afterwards be harmless.
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> "There is thus an essential difference between zymotic venom and a metallic poison like arsenic.
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> "In its weakest form *cholrine* produces diarrhœa in a great number of persons; but in every population a large number of people appear to resist its influence. They are insusceptible. The cases of attacks of the same person twice in this as in some other zymotic diseases are rare."

Speaking of the localization of cholera in East London, the report says it must have been diffused by personal contact; by translation through the air; by dissemination in vapour of sewers; or by the various waters.

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[1] Virchow's 'Handbuch der Speciellen Pathologie und Therapie,' 2 Band, 2 Abth., p. 382.

The different habits of our city populations and of the inhabitants of India cause a corresponding difference in the spread of cholera in England and in India.

As to the cause of the mortality by cholera in East London in 1866, we must be content with quoting a small portion of the investigations of Dr. Farr, by which he accounts for the difference of mortality—that in East London being seventy-two deaths in 10,000 living, and that in Chelsea, from the same cause, being but four in 10,000.

Reciting conflicting evidence given regarding the East London Water Company's Works, he says:

> "This was perplexing; but the company, by taking the unfiltered water from the open reservoirs, violated an Act of Parliament; and on the ground, perhaps, that they were not bound to criminate themselves, they, in the words of their engineer, besides pleading 'not guilty;' boldly proclaimed their innocence on August 1st. Shortly afterwards Mr. Greaves, however, admitted candidly that the open reservoirs of unfiltered water could be drawn on; and by the report of Professor Frankland (August 25th) goes a step further. 'Communication,' Professor Frankland says, 'can be established between these reservoirs and the pumping-wells supplying the public; but Mr. Greaves assured me that *it was never done except in case of emergency.*'
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> "In his examination before the Pollution of Rivers Commission, on December 10th, Mr. Greaves advances another step, and states that 'a small quantity of water was taken out of one of the open reservoirs this spring—he had not the date—probably June was the latest date;' but he could not say definitely. Further revelations were made by the workmen under examination by Captain Tyler; and the story is at length thus told by officers of the company:—The foreman of the company died quite suddenly of apoplexy early in this year (1866). In appointing a new man, in giving him discretionary instruction as to how he was to manipulate the water, 'I described,' says Mr. Greaves, 'this possibility (of drawing on the open ponds) as something which he must keep in mind, rather than suffer an accident; and so I suppose he may have acted in that sense.'
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> "He (Mr. Greaves) kept these open ponds in reserve, as the question of quantity was important, 'to meet such necessities as might arise.' The foreman, in his evidence, said *he kept no notes*, but believed that he 'allowed the water to run from the open to the covered reservoir not more than three times during the year 1866—at the latter end of March, at the latter end of June, *and some time in the early part of July*. The sluice was never opened after the cholera broke out. He is certain of that. *He had no fear of its doing harm.*'"

A carpenter, whose specialty it was to open the sluice, admitted that he frequently opened it in 1864 and in 1865, and three times in 1866. Dr. Farr then contrasts these statements of "these dependent men not giving evidence on oath" with those of Mr. Greaves, and observes, "it is enough to have in evidence, that immediately before the outbreak in July the foul water of the reservoirs was pumped over the parts of East London where cholera was epidemic."

"Again, there was another channel for the entrance of the foul water of the Lea directly into the covered reservoirs supplying the pumps." This was shown by Professor Frankland and Captain Tyler to have been rapid permeation through the foul and gravelly bottom of the reservoir. The absolute necessity for other than the legitimate sources of supply is also shown in the large quantity of water sent to the East London district by the Water Company.

The extreme gravity of the delinquency committed by the East London Water Company, which has been published in the report under review, requires more extended comment and publicity than we have given above; for this reason we will now quote that portion of Mr. Simon's 'Report to the Privy Council,' which speaks of that part of the Sanitary Act of 1866, which relates to water companies in relation to the public health.

He writes :—

"It has seemed to me, however—which I think it my duty to mention—that one important exception to this satisfactory state of of the law was brought to light in connection with last year's outbreak of cholera in London. I refer to the relations subsisting, and particularly in one respect, between commercial water companies and the public. It will be seen in Mr. Radcliffe's report that the East London Water Company, through its engineer, acknowledges to have acted in contravention of the fourth section of the Metropolis Water Act, 1852, by distributing for public use a water (and a most improper water) which had not passed through its filter-beds; and Mr. Radcliffe adduces very strong evidence to show that the East London outbreak of cholera was occasioned by this illegal and most culpable act. It seems to me that the public is hitherto very imperfectly protected against certain extreme dangers which the malfeasance of a water company, supplying, perhaps, half a million of customers, may suddenly bring upon great masses of population. Its colossal power of life and death is something for which till recently there has been no precedent in the history of the world; and such a power, in whatever hands it is vested, ought most sedulously to be guarded against abuse."

"It was precisely in the region of the Old Ford water-field that cholora raged; there, in three months, it killed little less than 4000 men, women, and children; while in the Lea Bridge field, and in all the other water-fields of London, the epidemic was kept within such narrow limits of fatality as would be accounted for by diffusion through sewers, direct contact with cholera matter in various ways, and the slightly contaminated filtered river-waters of the other companies."

The above quotation is from Dr. Farr's report. We will now consider the disease in reference to the London hospitals.

We have, in a previous number of the 'Review,' noticed the important contributions to our knowledge of the last epidemic afforded by the medical staff of the London Hospital, which stands in the midst of the district, or, as it is termed, water-field supplied from the Old Ford; of its 130 attendants, five died of cholera, a mortality of 385 in 10,000.

The first case in the London Hospital was admitted on the 10th July, the patient having arrived from Holland on the 6th July; she died on the 11th July. 509 cholera cases were admitted between the 10th and 30th August; 281 died, and 228 recovered. The mortality decreased from 85 per cent. in the first to 37 per cent. in the seventh week.

In Bartholomew's, Middlesex, and Guy's hospitals, collectively, 213 persons in cholera were treated, with a mortality of 79. Of other cases in the hospitals at the time but 2 took cholera, and they died. Of nurses or attendants in these hospitals, four took cholera and one died.

Guy's Hospital, in 1854, had an impure supply of water. 92 cholera cases were admitted and 19 persons, including attendants and ordinary patients, were attacked by cholera in hospital; of the total number of cholera cases 65 died. An eminent member of the staff died of cholera. In 1866, and for the last ten years, this hospital has obtained water from an artesian well.

Dr. Farr, reasoning upon the law of rise and fall of the epidemic, in 1849 and 1854, in comparison with what occurred in 1866, estimates that 10,375 less than the calculated number perished in the last visitation.

He argues the operation of cause and effect, the former being the admission of the cholera-polluted waters to the East London district, and the latter the rise of the cholera mortality. Again, when public attention was drawn to the matter and the engineer of the water company did his best to set the supply right, "the plague was stayed," and the mortality rapidly decreased.

"There is reason to believe that the period of incubation is as brief as the term of attack in fatal cases of cholera; the cholrine[1] often acts as suddenly as any of the poisons."[2] In reference to the rapidly fatal effects of the disease we find in table ix (third series, p. 116) of Report of Committee of Scientific Inquiry, that of 3,600 cases of cholera, 1,744 were fatal; and of these 734 died on first day, 428 on second, 186 on third, and 106 on fourth day.

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[1] Dr. Farr uses this term to designate the material substance that propagates cholera, he says, "simply as hypothesis."

[2] Dr. Farr here quotes Snow, Griesinger, &c.

Dr. Farr quotes the Newcastle-upon-Tyne epidemic of 1853 as closely analagous to that of East London in 1866. The narrative presents several points of painful but striking interest, corroborating that of the Broad Street pump.

At Newcastle-upon-Tyne in autumn, 1853, cholera broke out directly after the death of a woman who was attacked with diarrhœa at Bill Quay, but came by steamer to Newcastle-upon-Tyne, and died there on 1st September; the mortality rapidly increased till the 16th, when it amounted to 146 in 24 hours from a population of 144,067 including Gateshead. The average mortality was but 11 daily.

It was ascertained that the water supply contained sewage; this was remedied on the 16th September, the supply being only contaminated with what Tyne water remained in the reservoirs; in nine days the deaths had fallen to 81, and in six days more to 16 per day. By the 23rd November the mortality from cholera ceased. In 1866, 60 persons died from the same cause; the population had, at the latter date, considerably increased in Newcastle-upon-Tyne and Gateshead, as compared with 1853.

It is worthy of note that the working classes in Newcastle stated, they "could not drink the water;" they "were poisoned with the water." The mortality on the 15th and 16th September was at the annual rate of 3,625 in 10,000 living.

The report then details the causes which rendered the water supply worse in Newcastle on that occasion than even that in East London during the last visitation. It appears that the decline of the disease obeyed a similar law in both instances, upon the stopping of the impure supply; and this law is subject to certain influences from the fact that cholrine has the power of multiplying itself.

It is extremely important to note the following, which is in Dr. Farr's own words:—

> "While Newcastle and Gateshead, intimately united by the bridge, lie some twelve miles up the Tyne, the districts of Tynemouth (including North Shields) and South Shields lie on both sides of the lower part of the river to its mouth.
>
> Their general sanitary condition in 1853 was as bad as that of Newcastle, and it could scarcely be worse. The water was indifferent, but it was not drawn from the Tyne. The population, 104,750 in number, was in intimate connection, the 144,067 living in the Newcastle and Gateshead districts. Epidemic cases occurred; but in the

four successive days (September 14th—18th), when the mortality was so terrible in Newcastle, the deaths in Tynemouth and South Shields were, 0, 2, 5, 5, and 1; and in no single day during the month of September exceeded 8 daily. The disease was diffused more slowly, and in the middle of October (22nd) the deaths from cholera were 18, which the numbers never exceeded. The mortality by cholera in Newcastle and Gateshead for the year was 134, for Tynemouth and South Shields 24 in 10,000 inhabitants. Here the Newcastle water-field bore the same relation to Tynemouth and South Shields as the East London water-field bore to the rest] of London, except that in the northern districts the mortality was higher, and differed less proportionally in the two fields than it did in the metropolis, where the mortality by cholera in the East London water-fields was at the rate of 72 in 10,000, while it was 8 in the rest of London.

"These two great experiments yield analogous results. The parallel is complete; and it may now be laid down as an established law that water into which cholera dejections find their way produces cases of cholera all over the district in which it is distributed for a certain period of time, and that if the distribution is in any way cut short, the deaths from cholera begin to decline within about three days of the time at which the distribution is stopped."

The Broad Street pump is an instance of indirect diffusion of cholera by water, as are also the cases of the Southwark and Lambeth water supply in conterminous streets in 1854; this has been seen elsewhere also. Imperfect drainage the report evidences as another source of the disease, which, we may observe, has been long known. The water supply of London, containing within its police area more than three millions and a half of inhabitants, becomes a question of such vital interest that Dr. Farr does well in bringing it under notice in a work that treats so much of everything affecting the health of so vast a community.

This becomes still more evident when we consider that those who have recently studied the subject consider it right to calculate upon a source that would meet the wants of six millions of people, so rapid is the growth of the great British metropolis.

This supply Mr. Bateman proposes to draw from the Welsh mountains, storing the water in lake reservoirs, the lowest elevation of which is 450 feet above the sea, thence to be carried through aqueducts by gravitation, and distributed at such a pressure as to render unnecessary the use of fire engines in London. This, no doubt, would be a great boon; we must not, however, be led away from recollection of the fact that an absolutely constant supply is never attainable, owing to occasional intermission from accident and repair to water pipes, &c.; and as a'church that cost £20,000 has been burned down during some such intermission in the constant supply system, a much greater calamity might happen if we depended solely upon any constant system, however fairly entitled to the name.

We do not in the least desire to underrate the great benefit the supply of pure water would be in a sanitary as well as in a commercial point of view.

Lately also Mr. Hemans and Mr. Hassard have drawn public attention to the supply of London obtainable from Cumberland and Westmoreland. Mr. Dale claims this source of supply for the Lancashire and Yorkshire towns.

It would have been more candid if Mr. Hemans had acknowledged that the supply of water from the Cumberland and Westmoreland lakes had been first brought under his notice (as we understand it was) by a member of the medical profession, Dr. Charles Frederick Moore, eleven years ago, when the dry season of 1858 forced the subject upon public attention.

At that time the cost of carrying the water so far, the leakage, &c., were, Mr. Hemans considered, insurmountable obstacles to the plan. It is quite true the gigantic system of metropolitan drainage had not existence, and the public were not then prepared for such lavish expenditure upon engineering works of this nature.

Now, however, the water-closet system has so grown that every stream is thereby polluted, and the labours of the pioneers of modern sanitary science are becoming so confirmed by accumulating evidence, that the nation seems willing to endure an expenditure which would not have been assented to before, rather than use polluted water. Many scientific and practical men hold the opinion that not only the health of the nation but also its wealth are compromised, and the navigation of its rivers threatened by the present system of sewerage. And it becomes a matter of vital importance to determine how far we are justified in spending vast sums of money in the endeavour to counteract the many evils springing from the present plan, and whether, instead of propping it up, we had not better return to the original system laid down in the Levitical law, and in a certain form still practised in China and Japan, where, by its means, such extraordinary numbers of people are sustained in a comparatively small compass.

Mr. Fulton's idea is to draw the supply from the sources of the Wye; Mr. Denton's, from the highest sources of the Thames, coupled with the storage of surplus waters. The plan of Mr. Ormsby is to collect rain-water on glass, &c.

The attempt to enrich land by irrigation, and so to render innocuous the sewage of our cities, appears impracticable. In Dr. Farr's words—" Accepting the existing reign of the water-closet, the sewage-irrigation remedy for rivers as sources of water-supply is a policy of despair."

The sound policy of insisting upon having pure water for health sake is thus insisted on by the same author—"By a whole host of terrible diseases, God forbids the consumption of contaminated waters."

In a commercial point of view, those who have used pure water for washing well know its economy in the saving of soap, and in the lessened wear and tear of articles washed; for purposes of trade and manufacture the same rule applies.

In England and Wales, exclusive of London, 8782 persons died of cholera in 1866; in London the number was 5596; the proportion of deaths in London being 18 in 10,000, while in the rest of England and Wales it was but 5 in 10,000. We have before observed that the deaths in East London were proportionally vastly more numerous than in the rest of the metropolis.

The country has been divided by Dr. Farr into what he terms *cholera fields;* and in almost all parts of England and Wales the result of sanitary measures were seen to be highly satisfactory in the last epidemic. This was the case in Bristol, Wolverhampton, Salisbury, Hull, Sunderland, and other cities and towns.

In Liverpool, though the mortality was 54 per 10,000, Dr. Trench believes it would have been much greater had it not been for measures of disinfection promptly carried out; he "*very properly denounces the dreadful consequences of drunken wakes over cholera corpses.*" We may observe that the outbreak of cholera in the Channel Islands, and that in Dublin, in 1866, many times occurred at wakes.

In former epidemics *elevation of locality* was considered, with much reason, to govern the prevalence of cholera.

The generally improved water supply and sewerage, however, materially altered this relationship in 1866.

If we grant, with Drs. Farr, Snow, &c., that water carries the cholera flux, and that its power for evil diminishes every day it remains in the water, it is evident that *the further it is so transported, the weaker it becomes.*

Density of population adds to the fatality of the disease; and *imperfect sewerage* is another like agent.

Generally speaking, *wealth lessens, and poverty increases the probability of taking cholera.*

Occupations.—The greatest mortality occurred in those persons whose avocations obliged them to live in unhealthy localities; on this latter circumstance, rather than on the nature of their business, the relative mortality evidently depended.

Sex and age influenced the fatality of cholera; at all ages males suffered slightly more than females.

From the age of ten years to fifteen the mortality of both sexes was lowest; from that period it increased in "geometrical progression, the cholera meeting with less powers of resistance every year afterwards."

> "Small-pox, scarlatina, diphtheria, measles, and whooping-cough obey special laws of their own, yet all of them agree in this: the mortality by them declines as age advances; but the fevers and the other zymotic diseases taken in the aggregate are more closely allied to cholera, for the deaths by them are most numerous at advanced ages."

Men and women in the prime of life, in the reproductive ages, are nearly equally liable to attack.

The more destructive an epidemic is, the more rapid are the cases in their course.

Dr. Farr concludes that cholera matter may be very unequally distributed in large bodies of water like the Thames.

In the twenty-three weeks of 1866, during which cholera continued in London, the mortality was greatest on Wednesdays.

> "Heat, sunshine, clouds, humidity, winds, storms, rains, lightning, do not generate zymotic germs, but they promote or retard their growth and development."

The zymotic theory of cholera is discussed at some length. Dr. Farr speaks of germinal matter in the low form which he names cholrads—the "choleragenic molecules" of Pacini.

> "It is life in this low form, where it is developed in isolated particles at war with those which constitute men, that generates zymotic diseases. An epidemic is the war of a conquering host of innumerable particles of life. It is therefore subject to the laws of growth and decay, both in the individual man and in communities."

Air, water, and contact, have all had their advocates as the means of spreading the disease. The two latter appear to have been most generally regarded as those by which the disease extended in the British Islands.

The efficacy of preventive measures adopted in the army, navy, and the other branches of the public service, was strikingly evident in 1866.

> "Contagion has given to rise to passionate controversy, especially in connection with quarantine. The word still strikes terror into all the nations on the Mediterranean except the Turks; and vexing

quarantine is still their idol, although it for ever fails to protect them from violent eruptions of the disease."

"Now contagion is deprived of its terrors by the discovery that it acts through the agency of cholera flux in air or water, and when it actually enters the body only gives rise to disease in some cases, which can generally be arrested by prompt attention in the premonitory stage.

"The rigorous precautions to take are careful abstinence from contaminated water, the destruction of the flux, the disinfection of soiled linen, and in certain cases the removal of inmates from ships or houses. The isolation of the sick is generally unnecessary; but in a seaport they may be very properly treated in special hospitals for the reception of the patients suffering either from diarrhœa or cholera.

"The practice of England, where cholera has been kept within narrower bounds than in other countries, I humbly submit, deserves the serious attention of the people of the Continent."

Dr. Farr details the facts and speaks of the striking contrast afforded by our treatment of the cholera-smitten German emigrants at Liverpool, and the crews of the French fishing-smacks off Scarborough, in 1866, as compared with the "inhuman cowardice of Syra," where numbers were permitted to die of cholera and of starvation on board a ship rather than permit its freight of human beings to land at the lazaretto, or be supplied with food, or its dead to be buried.

The subject of quarantine is one admitting of being viewed in various ways; and in common with all who have travelled in Southern Europe and the Levant, we have seen the absurd and, indeed, cruel lengths to which it is often there carried.

We have seen a city deserted by its sovereign and chief officers of government, who carried away with them almost the whole medical staff of the country.

Dr. Farr recounts the horrible proceedings at Syra in the epidemic of 1865.

Mr. Simon shows the impracticability of carrying out a strict quarantine in a country of any commerce.

Dr. Farr depicts the far better plan of at once providing suitable hospitals for the sick, and suggests that the passengers be at the same time "cautioned, and supplied with remedies for the treatment of diarrhœa."

Those nations that practise quarantine, relying upon it, neglect the removal of "indescribable impurities," and, consequently, when the cholera does get access to the place, as in almost every instance it at length does, there is no abatement of its virulence. Spain, Italy, and France, are cases in point.

Mr. Simon quotes Sicily as an instance of, for a time, successful quarantine, till the cholera came with troops sent to quell the outbreak at Palermo. But had the people of Palermo met the disease as an enlightened people should have done, the infatuation which caused the outbreak would not have existed.

The cholera reached Guadeloupe by the soiled clothes of a boy (belonging to a French brig from Marseilles, who died of cholera on the voyage) being sent to be washed at that island, where one-ninth part of the whole population of the island died of the cholera within a short time afterwards. Strong evidence, some will say, of the necessity for quarantine; strong evidence, we would rather say, for the observance of common precaution in dealing with so potent a poison, and yet one that would have been so easily destroyed with the affected clothes.

An island near Guadeloupe was apparently kept free from disease by rigorous quarantine.

This 'Review' has, in a previous number, treated of the International Conference at Constantinople; it is, therefore, the less necessary to quote its final conclusions at present at any length. The first was (carried by a majority) that the traffic with the East should be stopped, except the mails, during the whole period of the epidemic.

The author of this article was present in Egypt during two most severe epidemics of cholera, and at neither period did the disease take that route to England.

Our nation's "influence" was "evidently perceptible in the importance" attached to hygienic measures by the Conference. "It demands for every man pure and abundant air, pure water, and a pure soil. The Conference believes that these elements should constitute the permanent privilege of populations," and should not be postponed until cholera epidemics threaten, or are in the midst of, a people.

Proper hygiene of ships, and the destruction of the chief source of the malady by chemical agents are insisted upon.

> "If the Indian Government has not been arraigned formally at the bar of Europe, it has been reminded of the responsibility which attaches to India as the source of the disease; and a large share of the measures proposed by this Conference are directed towards India and the cholera routes between India and Europe."

Dr. Farr's report goes on to say that this responsibility is not repudiated by Great Britain, and points to the energy her sons have shown in the acquirement of the vast empire in Asia, at the same time that he alludes to the settled habits of the natives of Hindostan, their theology, and pilgrimages, &c., all so utterly opposed to health.

That we have been in earnest in our efforts to improve that country may be seen in the good effects attending the sanitary improvement of the Indian Army, and in the works already begun in Calcutta, Bombay, and Madras.

Pettenkofer considers that the thickness of superficial porous soil which is left unoccupied by "ground-water," and, consequently, penetrable by air, determines the different behaviour of cholera at different times in the same place.

Mr. Simon alludes also to "some supposed successes, and some unquestionable non-successes, of chemical disinfection," where it had been used in the hope of preventing or arresting epidemics of cholera.

A third most important matter discussed at the International Medical Conference at Weimar "consisted in communications as to the so-called cholera fungus."

Mr. Simon claims for Germany and England what we know with any certainty of the pathology of cholera; he speaks of Boehm having, in 1838, stated,

> "That the whole extent of the intestine teems with a vegetation of micro-fungi; that innumerable round and oval, or more elongated, corpuscles are to be found in all the vomit and dejections, as well as in the canal, sometimes single, sometimes two, three, four, or more, joined end to end, as links of a chain, and these chainlets sometimes branching; that such forms are held together in mucous floccules, and come best to light when liquor potassæ is used; that within the small intestine they are often so numerous that not the smallest specimen will fail to show numbers of roundish forms amid the *débris* of epithelium. He appends an illustration, which shows quite unquestionably the forms of cryptogamic growth, and refers, for similar forms, to Schwann's then recently published investigations of yeast."

From 1838 to 1866 it would seem that cryptogamic forms like these were "almost never particularly observed." Dr. Parkes and other observers at home and on the Continent spoke of "peculiar corpuscles or granules," vibriones, &c., being found in the alimentary canal, or in the vomit and dejecta. Mr. Simon thinks much study is yet needed, both in health and in disease, in this branch of pathology.

The difficulties surrounding such investigations are very great; much also remains to be learned "in the general study of the zymoses." Dr. Aitken writes:

> "To say that these fungi are the cause of cholera would as yet be premature; nevertheless their existence is capable of accounting for many of the phenomena of cholera, and especially its spread.
>
> "The poisonous properties of fungi are well known, so that, apart altogether from the physical influence of such spores, by their development in the intestines, the products of their growth may be

eminently poisonous and deedly. In this direction the cultivation experiments may yet be carried."

Again—

"Professor E. Hallier, of Jena, whose reputation as a fungologist is not surpassed, has made numerous experiments by the cultivation of these spores; and from an able summary of his paper by Dr. Buchanan,[1] it appears that his observations were made on the stools of a person ill with cholera at Berlin in 1866, and on the stools and vomita of a cholera patient at Eberfeld in 1867. The characteristic vegetable elements consisted of a fine fungiform matter which floated, and of more highly-developed spore-cysts which sank to the bottom.[2] These spore-cysts were yellow or brownish bodies, consisting of a pale membrane enclosing highly refracting coloured spores. The cyst-wall undergoes a series of changes, ending in its rupture or solution, when the spores become free. The spores then, by progressive (*tomiparous*) partition, a process which may begin before they leave the cyst, resolve into very small cells grouped into balls and heaps, which Hallier calls 'colonies of micrococcus.' The small cells constitute the fungiform matter seen in the evacuations, and they attach themselves to any bodies there may be in the stool—to remnants of animal or vegetable food, to epithelium-cells, or to oil-globules, when these and all nitrogenous matters to which these fungi cells fix themselves become of a dirty aspect, and lose their structure," &c.

Besides the spores and minute cells, torula-like bodies were observed, and these were found to develope from the micrococcus-cells, and these latter mark a step towards the production of Oïdium forms of fungi.

At Eberfeld the stool and vomit contained less cysts and fungal colonies, and proportionately more free micrococcus-cells. As these latter increased in size the epithelium upon which they has fastened wasted. The parasitic fungi always grow at the expense of any nitrogenous organic substance which they attack, as seen in the fungus foot disease of India. We have not space to give at length the experiments of Hallier, or to quote the corroborative evidence of the Rev. M. J. Berkely, and of Dr. Tytler and other British physicians in India, who long ago connected cholera with a diseased condition of the rice-plant. Much is still wanting to complete our knowledge on this all important subject. To Asia is, however, attributable, by consent of all investigators, the origin of the disease.

A temperature below 54° Fahr., or above 144°, and free acidification of the suspected fluid, have been the means found most certain in preventing the growth of the fungi.

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[1] 'Ninth Report to Privy Council,' p. 512.

[2] Reference is here made to the engraved plate in Dr. Aitken's last edition of the 'Science and Practice of Medicine.'

Dr. Aitken quotes Budd, Pettenkofer, and Günther, to show that "cholera can do its very worst where the drinking-water can play no possible part in its dissemination."[1]

On the influence of an atmosphere contaminated by the effluvia arising from decaying animal matter and excreta, the exemption of the students in the Scottish dissecting-rooms from cholera, although the subjects in 1832 and 1848—9 were almost exclusively dead from cholera is cited by Dr. Aitken. The authority alluded to (Dr. Alison), however, attributes this immunity to what appears to be the case, namely, that cholera poison does not attach itself to the *dead* body (in a certain state of decomposition at least). For ourselves, we would not advise any one to trust too much to this alleged immunity.

This 'Review' has in former volumes, at some length, noticed the works of several recent writers on this disease; it is, therefore, not necessary to follow at any great length the subject of malignant cholera as given in the article on that disease in the fifth edition of the 'Science and Practice of Medicine,' which embodies much valuable information derived from Mr. Simon's ninth report, and other recent additions to the literature of the subject.

Urea is present in the blood after death during collapse, but not in the rice-water stools, which latter contain butyric acid, and yield nitrogen and carbonic acid. Urea seemed to accumulate after a protracted algide state; in bodies dead at an early period it was absent.

Death and desquamation of the epithelia pervades the entire surface where epithelium exists. In a note Dr. Aitken regrets the contradictory evidence as to the shedding of epithelium during life; in his experience the stools do not contain it; and

> "It is only in the post-mortem contents of the bowels (small intestine) that cylindrical epithelium is to be found—the result, no doubt, of maceration. It is alike found in the stomach, urinary, and gall bladder. Denudation of the intestinal villi, or desquamation of the epithelium, is no essential lesion of cholera."

Speaking of the brain, Dr. Thudichum observes—

> "The greatest demonstrable lesion of the brain is of a chemical nature. The amount of urea which may collect in it, and in the fluid which bathes its cavity and that of the spinal marrow, is truly astonishing.
>
> "The cerebro-spinal fluid contains sometimes a higher percentage of urea than the most concentrated human urine."

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[1] 'Brit. Med. Journal,' April, 13, 1867, p. 416.

Surgeon Adams, and assistant-surgeon Welch (who noted the last epidemic at Malta) speak of the baneful effects of drunkenness upon those ill of cholera, which habit appears, too, to predispose to the disease.

Dr. Aitken does not consider Dr. G. Johnson's castor-oil treatment of preliminary diarrhœa, when cholera is epidemic, warranted by what we know of the pathology of the disease. It is only when there is evidence of the presence of unwholesome, undigested food, or of a large and unnatural accumulation of the feculent contents of the bowel that he would give any castor oil. On the contrary, "Opium is the remedy which, by actual experience, seems most worthy of reliance."

The Epidemiological Society recommended special hospitals, or special wards in hospitals, with relays of a separate and special staff of attendants for the management of cholera patients.

Concluding Remarks.—Dr. Farr has laid considerable stress upon the propagation of cholera by water, and he is borne out in his opinion of its importance by evidence recently afforded by investigators in India.[1] The inquiries lately instituted into the prevalence of fever at Newcastle, as to its connection with the use of polluted water, also shows the importance of this subject. That a continuance of agitation on this subject is required is seen by the last-named occurrence having taken place notwithstanding the prevalence of cholera at Newcastle (1853) from, as it would appear, the same cause, namely, the use of sewage-polluted water.

We do not desire for a moment to exclude from closest attention the other agencies engaged, probably, in the spread of cholera,—as the use of foul linen, and vessels, and by the hands of attendants upon those already sick of cholera, its propagation in a volatilized form, in sewer vapours, and by the so-called fæcal odour, in filthy and over-crowded dwellings, &c. The microscopical investigations as to the fungoïd causation of the disease promises to afford most important knowledge; at the same time, revolting as the idea is, it would be highly improper not to make known in every way the all-important fact, which is thus expressed by Mr. Simon:—

> "It cannot be too distinctly understood that the person who contracts cholera in this country is *ipso facto* demonstrated with almost absolute certainty to have been exposed to excremental pollution; that which gave him cholera was (mediately or immediately) cholera-contagion discharged from another's bowels; that, in short, the dif-

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[1] See 'Lancet,' February 20th, 1869.

fusion of cholera among us depends entirely upon the numberless filthy facilities which are let exist, and specially in our larger towns, for the fouling of earth, and air, and water, and thus secondarily for the infection of man, with whatever contagion may be contained in the miscellaneous outflowings of the population.

"Excrement-sodden earth, excrement-reeking air, excrement-tainted water—these are for us the causes of cholera.

"That they respectively act, not only in so far as the excrement is cholera excrement, and that cholera excrement again only acts in so far as it contains certain microscopical fungi, may be the truest of all true propositions; but, whatever be their abstract truth, their separate application is impossible.

"Nowhere out of Laputa could there be serious thoughts of differentiating excremental performances into groups of diarrhœal and healthy, or of using the highest powers of the microscope to identify the cylindro-tænium for extermination. It is excrement, indiscriminately, which must be kept from fouling us with its decay."

Mr. Simon then repeats his oft-repeated practical advice, namely, that all excremental produce be so quickly and thoroughly removed that the inhabited place, its air and soil shall be absolutely free from fæcal impurities; and that the water supply shall be preserved from contamination of a like sort. Mr. Simon forcibly also points out the constant mortality from typhoïd fever and diarrhœa by the same influences.

Our present limits prevent our noticing any of the many other points of deep interest contained in the works under consideration in reference to cholera, which we have not touched upon.

The works of Mr. Simon and Dr. Farr contain maps and diagrams of great value; Dr. Aitken's work is illustrated with representations of the so-called cholera fungi; and Dr. Thudichum's contribution to Mr. Simon's work is illustrated by coloured spectrum representations.

PART SECOND.

Bibliographical Record.

ART. I.—*La Médecine dans Homère.* Par CH. DAREMBERG. Paris, 1865. Pp. 96.
The Medicine of Homer. By CH. DAREMBERG.

THE indications contained in the Homeric poems of the state of medical and anatomical knowledge in the early period of Greek civilisation have a double interest for the scholar and historian, and for the scientific enquirer. They began early to furnish a subject for set treatises. The earliest attempt of the kind mentioned in Dr. Daremberg's list is a medical thesis at Paris; the date of time is given by him as 1670, but obviously by a misprint for 1570. The next in date mentioned by him is a larger and more systematic work by an Italian physician, Persona, of Bergamo, published at Venice, 1613. Both these attempts partook of the imperfections of the science and scholarship of their time. The Germans of the last and the present century took up the subject, but apparently with no great success, unless some of Welcker's papers may be considered an exception; but Welcker's strength was in his scholarship rather than his scientific knowledge. M. Malgaigne appears to have been the first in recent times to bring adequate professional acquirements to the study and illustration of the Homeric notices on medicine and surgery. Dr. Daremberg speaks with much respect of his predecessor's researches, which have been, he says, the *point de départ* of his own. But he is not satisfied with M. Malgaigne's conclusions on various points, and, we should imagine, he thinks more highly of his ingenuity and of his knowledge as a man of science than of his thorough and scholar-like acquaintance with Homer. In Dr. Daremberg himself both qualifications are united in a remarkable degree. Living in the midst of the medical science of Paris, he is, what has been till lately a rare thing in France, though it is, we believe, becoming less uncommon now, a really learned Greek scholar, and he is, probably, one of the first authorities in Europe on all matters connected with the literature of ancient medicine.

His essay seems to exhaust the subject, and takes account, probably, of every passage and word in the Homeric poems which bear on his theme. He brings to a focus all the stray lights in them which tell us anything of the physicians, the anatomical knowledge, the physiology, the surgery, and the medicine of those early days. He is not inclined to think that we can go much higher than the earliest periods of Greek civilisation for the origin of medical science. It is not certain, he says, that oriental medicine, in any approach to a scientific shape, was older than the Greek; and in the next place, unlike the Greek, it has been the germ and stimulus of nothing beyond itself. "Our written medicine," he says, "flows, as from an inexhaustible source, from Greece." The Homeric poems represent a state of society in which many steps of civilisation, and some of the most important and characteristic ones, had been already taken; and among them, the functions of the healer of bodily evils had been distinctly recognised, and the attentive and discriminating observation of the structure and general laws of the human organization had made considerable progress. M. Daremberg dwells on the fact that the office and services of the "healers" Machaon and Podaleirius, in the Iliad, are so prominent, and contrasts this with the absence of any notices of the same kind in the military history of much more recent times, as among the Romans. Even the immortal gods have their special healer, Pæon, still distinct in Homer from Apollo, and, of course, much more from Asclepius. M. Daremberg notices, though he warns us against laying stress on the circumstance, that no "healers" are mentioned among the Trojans. The most important part of his paper is what may be called a medical glossary of Homeric terms. He collects and defines, as far as he can, every word in the Iliad, Odyssey, Hymns, and Battle of the Frogs and Mice, which has any anatomical or physiological significance. The collection is an important one; "for," he remarks, "the Homeric language is singularly rich, and for its age often curiously correct, in this matter;" and further, "the nomenclature of the Iliad and Odyssey has continued to be the scientific nomenclature of the Greek physicians, and by them has reached to our own days." From Homer to Hippocrates anatomy made little advance; the poet's language sufficed for the use of the great medical teacher and his schools till methodical dissection began at Alexandria; and, in the absence of experimental research, this language was surprisingly full and exact.

"Cette richesse de langage, ces notions quelque-fois précises sur la place qu'occupent soit les viscères, soit d'autres organes, cette détermination exacte des régions dangereuses,ce discernement si juste des chances de salut ou des chances de mort, supposent une tradition médicale et une habitude de l'observation. Sans doute on ne disséquait pas au temps des rhapsodes, mais déjà on avait mis à profit tout ce que la vie domestique et le hasard des batailles peuvent révéler sur la structure des animaux et de l'homme."

M. Daremberg's collection of terms is a novel and valuable contribution, not only to the history of anatomy, in which it gives us the earliest efforts at distinct specification and terminology, but to the correct knowledge and definition of a characteristic portion of the poet's language, and, as far as we have examined it, appears to be very complete, and to be executed with the cautious and conscientious modesty which befits a subject in which the most learned man will most feel the imperfections of his knowledge. The special merit of M. Daremberg's attempt at definition is that he keeps fully in view the necessary generality and vagueness of such language when appearing in a popular form, yet, without pressing it to an improbable and forced nicety, he shows how it bears evidence to the clear and keen perceptions of the race which used it.

Next to this glossary the most curious portion of Dr. Daremberg's paper is his classification of wounds. He mentions 141 separate kinds of wounds, definitely fixed to particular parts of the body, and he observes that the gravity of each and the chances of life going with each are clearly known and discriminated. With this the most material and full portion of his survey comes to an end. Of physiology proper, of diseases, of medical remedies and treatment, of the surgeon's craft and devices, the Homeric poems have much less to tell. We learn more of the application of bandages and so forth from representations on Greek vases, of which Dr. Daremberg has engraved some examples, than from anything that the Homeric poems teach us. M. Daremberg brings together all that is to be found; but it does not come to much. A question has even been raised, whether Homer bears witness at all to the existence of the physician, as distinguished from the surgical healer of wounds. Dr. Daremberg thinks that he does, and controverts the opposite view of his predecessor, M. Malgaigne. But his arguments hardly go further than to meet the negative presumptions of M. Malgaigne. M. Malgaigne argues that as the cure of disease is, unlike wounds, always referred to a divine power, there could have been no recognised physicians. M. Daremberg answers justly, that in the days of Hippocrates, and as late as Roman times, when there were physicians, it was common to speak in the same way. But he urges, as a positive proof of the recognition of the physician's business in the Odyssey, the mention of him in the speech of Eumæus (xvii, 383), who encounters, among the wanderers whom men ask into their houses, *τῶν οἳ δημιοεργοὶ ἔασιν,* the *ἰητῆρα κακῶν*—

"Those who in some craft excel,
Framer of spears, leech, seer, or bard divine."

"Who," he asks, "is this healer of evils?" And he urges that the word cannot mean a healer of *wounds*, but contains "a manifest allusion to internal medicine." This seems to us to be pressing words too far. If the word have this meaning, it is from the intrinsic probability that among people such as those described in the Odyssey, with their builders, and artists, and poets, the art of the physician must have appeared, rather than from the impossibility of confining the terms ἰητῆρα κακῶν to a healer of wounds and fractures. But the phrase can hardly be taken as a positive and unambiguous proof of the distinction existing between "internal medicine" and surgery. To all who are interested in the subject Dr. Daremberg's paper will be very acceptable; its good sense and simplicity are equal to its careful learning, and it has the advantage of being concise, succinct, and unencumbered with any needlessly diffuse comments or digressions.

ART. II. *On certain Moral Aspects of Money-getting.*—By W. T. GAIRDNER, M.D., Professor of Practice of Medicine in the University of Glasgow. Pp. 47. Glasgow, 1868.

DR. JOHNSON'S saying, that "there are few ways in which a man can be more innocently employed than in getting money," though it had perhaps a touch of satire in it, was meant on the whole in earnest, and is a sound one; but all who believe the morality either of Paganism or Christianity will admit that it asks both for qualification and explanation. Dr. Gairdner in this pamphlet, which was in its original shape an address to an association of young men mostly in business, attempts to give these. His question is, under what condition does trade—that is, direct money-making—escape, as it is plainly the fact that it does, in numberless instances, in modern society, the condemnation of being a service of Mammon, and prove itself capable of going on in harmony with the morality of the New Testament? The fact that it is so capable he assumes, as, of course, he is entitled to do, with the experience of the numberless noble shapes which the merchant's calling has taken under the influence of religion and a high morality; and the explanation of it he finds in the idea, characteristic of Christian times contrasted with the civilisation of the old world, that wealth, and the trade of getting it are to be regarded as one among the manifold and diversified ways of concentrating and turning to account, for the service of mankind, the powers and opportunities which are given in the world as we find it.

The "master-key" to the inquiry is to be obtained by "viewing wealth, not as a mere power or force, but as a ministry or service;" and this view, he adds, is one "peculiar to modern times, and one which may be regarded as practically unknown to the ancient world." Of course he is far from forgetting the imperfect degree in which this principle has kept in check the falsehood and wickedness which still so frightfully taint trade; but he points out that in the ancient system of ideas "the whole structure of society was deeply pervaded by the relations of master and slave, conqueror and conquered; and property was essentially bound up with the idea of *mastery*." The opposite idea dates from that life and teaching which made the highest work of man, not "to be ministered unto, but to minister," not to be great, but to serve. This proclamation of the "essential dignity of *service*" "touched a central truth in the heart of humanity," and silently revolutionised the aspect and spirit of the employments of man in society.

Viewed in this light, as an employment followed as a *service*, trade and money-making rise out of the low grade in which the feelings of the old world placed them, as, without this animating spirit, they at once fall back into their old place.

> "It is possible," he says, "it is strictly just, to claim for commerce a noble position among the occupations of men; but if I have rightly interpreted the moral phenomena, so to speak, of commercial life, this high position is assigned to it, not in virtue of its gains to the individual man, but in consideration of its services to mankind—not, in other words, because it enables large profits to be made, but because it enables good works to be done, and, in the case of the individual man, supplies him at once with a function and a motive—a place in the world's army of progress, a definite portion of the world's work to be faithfully performed. In proportion as this ideal is fulfilled, in proportion as the idea of *service* overpowers the idea of *profit* in any business, will be the inherent nobleness of that business, the degree to which it will rise out of the dead level of money-making pursuits into the nobler and freer life of a *profession* or a calling."

This is very well stated, and it is undoubtedly true that, however imperfectly this ideal is realised, it is an idea which is before the minds of men, and which acts in arresting the corruption which would otherwise be inevitable in communities in which business and riches increase as they do at present.

> "In getting, no less than in spending, the service rendered, the good done, may have been made the first consideration, the pecuniary reward the second;" and so "accumulated wealth and capital may then become a new obligation to the service of humanity." "In this, political economy and Christianity converge to one point."

Hence, Dr. Gairdner lays down, as his leading practical rule, that a man of business ought to make his habits and ideas about his business approximate as much as possible to those of a profession. A profession brings a man money, and it is followed that it may do so; but if a man thinks first and most of the money he makes by his profession, not only is his profession degraded, but, in the majority of instances, he will actually miss the aim and results of the profession which he degrades and injures; and this indirectly reacts on his gains. The artist or the professional man, however much he may think of the gains of his work, must feel that in his art or his profession there are great ends to be sought for themselves, or he will make but a poor thing of his work. The same law does not hold in direct money-making; all wholesome and worthy money-making more or less aspires, as Dr. Gairdner says, to assimilate itself to the aims and feelings of a profession. But in drawing a distinction between fair and legitimate trade and gambling, Dr. Gairdner seems to us to fall into some questionable political economy. Starting from Adam Smith's distinction between "value in exchange" and "value in use," he insists upon "value in use" as the essential material of legitimate trade:

> "It is only when the whole attention is absorbed by the purchasing power, or economic value, of commodities, that the moral character of transactions utterly disappears."
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> "The morality of an exchange depends essentially upon the fact of its being a benefit to both parties concerned in it. The profit must be on *both* sides, and not on one only, in order to be a sound and moral profit."
>
> "The essential character of a gambling transaction, as opposed to fair trading, consists in the fact that, while *the trader seeks to make a fair profit through the profit of his neighbour*, the gambler seeks to realise a profit out of the loss of his neighbour, singly or collectively, according to the nature of his hazard."

This seems to us to be an unreal way of putting things. The fair trader in each particular transaction has nothing whatever to do with his neighbour's profit. He has not "to seek for profit through his neighbour's profit." It is too much to put upon him the care of his neighbour's profit, except so far as it depends on his own fairness and integrity in the bargain. In each separate dealing the only moral element that can come in is the honesty and justice which secure the fulfilment of its conditions. The higher morality of a business must be determined by its general tendency to benefit the community or the world, and by the degree in which this aim governs and directs its operations; not by the consideration, in each separate "exchange," that there should be a "profit on both sides." A man who means to make his neighbours lose is, of course, a gambler and something worse, but the fair trader is not bound to see to it that they do not.

Dr. Gairdner's distinction belongs to the general character and tendency of a business, and not to its particular "exchanges." An honest man would not choose a business in which he could only gain by his neighbour's loss; but given a business fair in itself, the only right and healthy view is, that in each transaction or exchange, subject to the rules of truth and justice, each side should look after itself. The difference is an important one, and he has not made it clear; but his pamphlet, though, owing to its original purpose, it is sometimes rather too rhetorical, is, on the whole, a thoughtful and honest and essentially sound attempt to give a reasonable answer to an important and sometimes perplexing question.

ART. III.—*Observations on Amputation of the Thigh, and on the merits of that Operation as compared with Excision of the Knee.* By WILLIAM MCCORMAC, M.D., one of the Surgeons to the Belfast General Hospital. Dublin, 1868. Pp. 20.

THIS pamphlet is a reprint of an article by Dr. McCormac, in the 'Dublin Quarterly Journal of Medical Science,' August, 1868, and it is one to which we would desire to direct our readers' attention, since the important subject which it treats of has recently received fresh illustration from the ample experience of Dr. Humphry and Mr. H. Lee, as communicated to the Royal Medical and Chirurgical Society at their first meeting this session. We shall make no apology for re-opening the oft-disputed question of the relative danger to life attending on amputation of the thigh or excision of the knee. Far from being settled, the question of the desirability of excision as a frequent operation for the relief of disease of the knee must, even at the present time, be still judged of very differently by surgeons of equal eminence and experience if Dr. McCormac's facts are to be trusted. He says—"In the London Hospital, Mr. Curling informs me, the operation is not much in vogue. During the last four years resection of the knee has been only twice performed. In Guy's, Mr. Bryrant writes to me to say that, so far as he can learn, excision of the knee for disease has only been three times performed." On the other hand, Dr. Humphry was able to adduce thirty-nine cases from his own practice at the comparatively small Addenbroke Hospital at Cambridge, and the operation has been performed at least forty times at King's College Hospital. Now, at Guy's and the London Hospital together there are about ten times as many surgical beds as at King's College, consequently the operation is at least fifty times as common at the latter as at the former hospitals. The present reviewer pointed out some years since this difference in surgical opinion as to the worth of excision of the knee;[1] and the facts stated by Dr. McCormac shows that it still exists, notwithstanding subsequent additions to our experience. The question is one of the most important and of the most practical that can be mooted in operative surgery. If it were indeed true, as stated by Mr. Butcher, and as might perhaps be inferred from some words used by Dr. Humphry in his recent paper, that excision of the knee is a slight operation, much less dangerous than amputation, would it not be an unavoidable inference that excision should be introduced in the place of amputation in those cases of affection limited to the structures of the joint where amputation now proves commonly unsuccessful? Such affections are, speaking generally, recent injury, acute abscess, and chronic disease, where the latter occur in persons of advanced life and of broken constitution. Is it not notorious, on the contrary, that in these classes of cases excision is almost uniformly fatal? In fact, if we examine the recent statistics of excision a little more closely, we shall see that they are founded almost entirely on operations performed for chronic disease in childhood, though in Dr. Humphry's list there is a fair proportion also of cases of young adults operated on for chronic disease. Now, the experience of every operating surgeon will show that in childhood amputation for chronic disease of the knee hardly ever proves fatal. Nor are there wanting records of amputation of the thigh performed in the country with greater success than is shown by Dr. Humphry's account of resection of the knee. Thus, in the Plymouth Hospital, in a continuous period of fifteen years, of thirty amputations of the thigh, performed for disease not stated to have been chronic, though probably in the great majority of cases it was so, only two died.[1]

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[1] 'Syst. of Surg.,' iii, 818. See also the number of this Journal for July, 1862.

It cannot be kept too steadily before the minds of those who handle statistics that they are of no use for purposes of comparison unless the things compared are similar. To compare, as has been done in the matter before us, the mortality after excision mainly in childhood and mainly for chronic disease, with that of amputations performed at all ages and for all classes of causes, is perfectly illogical, and can lead to no sound conclusion. The more we see of excision of the knee, both in children and adults, the more thoroughly are we convinced that it is more dangerous than amputation of the thigh. This is quite consistent with a lower mortality after excision than after amputation,[2] if excision is practised in a more favorable class of cases. Now, we ask, is it not notorious that this is really the case? In the debate which followed the reading of his paper at the Med. and Chir. Society Dr. Humphry, with his usual frankness, tacitly allowed that some proportion of his cases were probably curable by rest, but said—and with very great truth and reason, in our opinion—

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[1] Report on Hospitals by Dr. Bristowe and Mr. Holmes, in the 'Sixth Annual Report of the Med. Officers of the Privy Council,' p. 665.

[2] We do not mean to admit that it has ever been shown that the average mortality after excision has been less than after amputation, but merely to point out that this would not be in itself conclusive unless it could also be shown that the cases were similar.

that time was an element of the question in the case of a working man. This view of the case, however, though it may amply justify the operation, does not place it on the same level as amputation, which is hardly ever undertaken except after all other means of treatment have been exhausted and the patient's health has visibly begun to fail. When excision is performed under parallel circumstances it is, as far as our experience extends, an extremely unsuccessful operation. The experience of Mr. Le Gros Clark points to a similar conclusion. Dr. McCormac says—"The practice of Mr. Le Gros Clark has been attended with striking success. He has operated a dozen times. In only one instance, that of a strumous child, was the operation primarily fatal." After alluding to another case, in which death ultimately resulted, Dr. McCormac quotes from a letter of Mr. Clark the following words—"I think there is no doubt that early interference by operation favours success, but I am also satisfied that protracted rest will cure many cases where excision might get the credit if performed" (p. 16). This again shows that, in Mr. Clark's practice, excision has been resorted to in cases very different from those where amputation is practised.

The conclusion which seems to us necessitated by the information before us is that excision of the knee may be practised with moderate success[1] in disease of a chronic nature, occurring in children and young adults of sound constitution and temperate habits, but that it is a severer operation than amputation, and one which must be reserved for cases in which amputation is judged unjustifiable on account of the probable curability of the disease.

This conclusion is repelled by some of the advocates of excision as though it involved condemnation of their favorite proceeding. It really, however, involves no condemnation whatever. The present writer, who has incurred much wrath from the ardent excisionists for expressing and defending this opinion, is yet as staunch an advocate and as diligent a practitioner of excision as any in the profession. It is precisely in cases such as those indicated that excision finds its appropriate sphere—cases in which, before the introduction of this operation, surgery was almost powerless. In such cases recovery may be fairly anticipated from years of constant and intelligent attention. Hence the surgeon is loath to recommend amputation, and the patient or his parents yet more loath to submit to it. But the result is after all uncertain, and often very imperfect, as far as the use of the limb is concerned. Besides, the poor can hardly obtain the constant care and prolonged rest which this plan requires; and, meanwhile, how are they to live? Excision holds out a hope of quite as good a limb as is obtained after natural anchylosis, and a restoration to activity in a very short time after quitting hospital. The above considerations seem to us amply to justify and recommend the frequent performance of excision of the knee in hospital practice, and at the same time account for its rarity in private practice among persons in easy circumstances.

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[1] We say moderate success, for up to the present time we have seen no reason to think that the success of any operator who has practised excision of the knee on a large scale has been equal to that which he would have had if the very same cases had been amputated—that is, if we use mortality as the only test of success. Thus, in the debate on Mr. Lee's and Dr. Humphry's papers at the Medical and Chirurgical Society, Mr. T. Smith said that he had excised the knee in fourteen cases, with only two deaths, Now, as Mr. Smith's cases were, we believe, all in childhood, and as in childhood amputation for chronic disease is almost always successful if performed while the child is in good health, we do not doubt that amputation would have shown a still better result in this particular. We do not say this in depreciation of Mr. Smith's practice, for the writer was himself consulted in most of these cases (being at that time Mr. Smith's colleague at the Children's Hospital), assented to the course proposed, and would have followed it himself had the patients been under his own care.

Another matter which is forced on our notice by this controversy about the relative mortality of excision and amputation is the value of the statistics of surgical operations. We have no hesitation in saying that in the bald way in which these statistics are usually collected they have very little sanitary meaning and scarcely any scientific value. Thus, in the official returns of the Parisian hospitals, the figures which indicate the number of amputations or other operations performed, and the number of deaths (followed by a grave statement of the per-centage even in the case of the smallest numbers) are returned, but without any particulars. The only value which such bare returns can have is that, if the deaths are very numerous, some unfavorable influence may be suspected; but whether in the hospital arrangements, or in the surgical treatment, or in the class of cases, remains quite uncertain. For purposes of comparison they are useless. Some authors, however, deal with the statistics of operations as though operations were independent entities—as though, for example, in a case of disease or injury, if amputation were performed, a different animal, as it were, would be substituted, whose natural history could then be traced without any reference to what preceded its birth. When the absurdity of this is pointed out refuge is generally taken in the vague plea that errors will be corrected by large numbers. Such would, no doubt, be the case if the errors were accidental, but it is clear that if they proceed from any general cause they will only be aggravated by increasing the number. Thus, of two operations, one of which, though really the more severe, is more rarely followed by death, because performed in milder cases, the error (resulting from omitting the indications for operation from the statistics) of regarding the severer operation as really the milder is, of course, increased by every 100 of each class collected.

Meanwhile, however, any contribution of the results of individual experience and opinion, if fully and intelligently recorded, is most useful, and we have no hesitation in classing Dr. McCormac's pamphlet as one which will greatly assist in forming a sound and unbiassed judgment on the point in dispute.

ART. IV.—*Beretning om* 100 *Tilfælde af Bændelorm hos Mennesket, iattagne her i Landet.* Meddeelt paa Prof. A. HANNOVERS og egne Vegne af Dr. H. KRABBE. Aftryk af Ugeskrift for Læger 3 R., VII, 1869, Nr. 7. Kjöbenhavn, 1869. 8vo, pp. 15.

Report on 100 *Cases of Tapeworm in the Human Subject, observed in Denmark.* Communicated on the part of Prof. A. HANNOVER and of himself, by Dr. H. KRABBE. Reprinted from the 'Ugeskrift for Læger,' 1869. No. 7.

THE above most recent contribution of Dr. Krabbe to the subject of helminthology, in which department he has been for so many years a successful and indefatigable worker, consists of a statistical report, based chiefly upon contributions of tapeworms sent in from time to time by physicians in different districts of Denmark, in answer to a request to that effect expressed by the author and his colleague about seven years ago. The subject is one which, from the great prevalence of tapeworm in certain parts of the world, and from the amount of suffering to which the disease gives rise, is important and interesting in both a practical and pathological point of view.

"It is not many years," observes Dr. Krabbe, "since it was generally supposed that in this country only one kind of tapeworm was found in man—namely, the *Tænia solium.* At that time the *T. mediocanellata* had not been distinguished from it, and the *Bothriocephalus latus* was considered to belong almost exclusively to Russia and Switzerland. But it now appears that all these three varieties occur among us, although, no doubt, the *T. solium* is the most common; and that, moreover, the *T. cucumerina* (or *elliptica*) is also sometimes met with here as in other countries. Of the 100 cases, 53 belong to the *T. solium*, 37 to the *T. mediocanellata; T. cucumerina* (*elliptica?*) was observed once, *Bothriocephalus latus* nine times."

The author appends a table exhibiting, in the 100 cases referred to, the sex, condition in life, age, place of birth, place of abode, remedies employed, length of the worm, name of the contributor of the specimen, date of the expulsion or reception of the specimen, and a column of general remarks.

"It is not unimportant, both in an etiological and a prognostic point of view, to be able to decide what kind of tapeworm we have to deal with; and the most convenient mode of rendering the peculiarity of the parasite conspicuous is the following:—Some of the posterior joints are placed, if the worm has been preserved in spirit, in a solution of caustic potash, until, in the course of a short time, they become transparent. They are then washed with water, spread out on a piece of glass, and allowed to dry. The position of the sexual openings, and the ramifications of the egg-receptacles, so far as they contain eggs, thus become very evident; and it is only the distinguishing between the *T. solium* and the *T. mediocanellata* which can then occasionally present any difficulty. If the tapeworm have not lain in spirit, it is unnecessary first to treat it with potash.

"*T. solium*, as well as *T. mediocanellata*, has irregularly alternating sexual openings, the latter being now found on one and again on the other edge. It is in all its measurements smaller than the *T. mediocanellata*, the egg-receptacle has fewer ramifications, and the head bears a short nozzle with a circlet of hooks. It is developed from the measles of the pig, *Cysticercus cellulosæ*, and presents the danger that it may also infect its own host with *Cysticerci*.

"*T. mediocanellata*[1] is, of all the tapeworms occurring in the human subject, the most difficult to expel. The feeding experiments made, first by Leuckart, and subsequently by others, have shown that its egg is developed by a *Cysticercus* in the ox; but it was Knoch ('St. Petersburger med. Zeitschrift,' Bd. x, 1866), of Petersburgh, who first succeeded in demonstrating the spontaneous occurrence of this *Cysticercus* in market cattle."

These two tapeworms present a great analogy in their mode of development. Man becomes liable to them from eating raw or underdone meat, pork or beef. From the author's tables it would appear that they occur most frequently between the twentieth and fortieth years of life, and that, on the whole, they are met with more than twice as often in the female as in the male sex.

"Usually there was only one tapeworm, but in four instances there were two, and in that case they were always the *Tænia solium*. This is scarcely accidental, for it is probable that *Cysticerci* are present in smaller number when they occur in the ox than when the pig is affected with them. Kousso, fern, and pomegranate roots were the remedies most frequently employed, kousso especially against the *T. mediocanellata*" (p. 13).

The *Tænia cucumerina* is the tapeworm which occurs most frequently in the dog. It is much smaller than the *T. solium*. The elliptical mature joints often have a reddish colour, due to the eggs collected in groups, and the sexual openings are opposite, being found on both edges of every joint. Whence the dog derives it is not known; Leuckart suspects that it is from insects, and man may then probably obtain it from the same source, and not directly from the dog. It is difficult to distinguish this variety from the *T. elliptica* of the cat, and, perhaps, both occur in man. It is chiefly in infants that it is met with in the human subject; thus, Hr. Rosenstand observed it (or *T. elliptica?)* in a child of three months, and Hr. Friis saw it passed spontaneously by one of seven weeks. As it does not attain to any great size, and seldom occurs in man in large numbers, it is not likely to have any special influence on the health; in the dog it is often met with in hundreds, sometimes in thousands.

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[1] The distinctive characters of the *T. mediocanellata* are described in our 35th volume, p. 341.

The *Bothriocephalus latus* has, as is well known, the sexual openings on the flat surface of the joints. Much more striking than this is, however, the most frequently bluish knot in the middle of each joint, proceeding from the egg-receptacle filled with eggs; in the mode of preparation above described this readily becomes visible as a dark brown rosette. As to the development of this species, it is known with certainty only that the young which issues from the egg, is, as in the *Tæniæ,* provided with three pairs of hooks, but has besides, a covering of cilia, by the aid of which it swims about for a time in water. It is therefore to be expected that man derives this parasite from freshwater animals, which the hooks enable the young to pierce, rather than from the drinking water itself; but the mode of transference has not as yet been satisfactorily investigated.

ART. V.—*Physical and Medical Climate and Meteorology of the West Coast of Africa, with Valuable Hints to Europeans for the Preservation of Health in the Tropics.* By JAMES AFRICANUS B. HORTON, M.D. Edin. 1867. Pp. 321.

FROM an Address to the President and Members of the Executive Council of the Central Government of Western Africa with which Dr. Horton prefaces his work, his object in publishing it appears to have been twofold: to quicken the somewhat dilatory steps which the Council had initiated in the way of sanitary reform throughout the colony, by pointing out what are its real sanitary condition and necessities; and to enlighten the intending emigrant to those not very attractive shores on the various maladies to which he would be exposed. Looked at in either light, such a work as this must be regarded as having more than usual claims to the favourable consideration of the critic. The climate of Western Africa, and especially that of the British possessions on the coast, enjoys such an unenviable reputation for insalubrity that the preservation of health necessarily occupies a primary place in the thoughts both of intending and actual residents in it. Unlike the East and West Indies, in both of which the Europeans may avoid most of the endemic diseases of the country by a very ordinary amount of caution in the matters of regimen and exposure to heat, the dweller on the coast of Africa is constantly exposed, from the alluvial nature of the soil, and from the rank luxuriance of its vegetation, to malarious influences which so lower the energies of the constitution as to greatly impair its powers of contending with the more violent epidemic manifestations of paludal poison which at recurrent intervals sweep over them. In these localities the existence of the white man is, to use a term which has now become an ordinary one, a continual "struggle for life," and it is only by the severest regularity of diet and habit, aided by the employment of all the hygienic precautions which scientific medicine can dictate, that he can hope to carry on for any length of time the unequal conflict. Nor does preservation of health under these circumstances depend wholly upon individual care, since experience has abundantly shown that the most scrupulous exactness of life is, at times, of little avail in warding off the attacks of those forms of disease whose intensity in urban populations is greatly exaggerated by the influences which always attend ochlotic gatherings. If a system of sanitary supervision be necessary in the towns of Europe, it is ten times more so in those of Western Africa, where all the conditions of epidemic disease are infinitely more active than they are with us. Dr. Horton's work supplies us with ample proof, if it were necessary, that no small portion of the mortality which decimates the inhabitants of Western Africa is due to visitations which, if not entirely preventable, might, at least, be greatly mitigated by enlightened prophylactic measures on the part of the local authorities. Whether the introduction into the colony of Western Africa of a rigid sanitary reform would lead to its becoming, as Dr. Horton seems to anticipate, one of the healthiest of Her Majesty's inter-tropical possessions, is questionable, but that a great amelioration of the present rate of mortality would ensue is beyond doubt. To this good cause Dr. Horton has made a valuable contribution by the publication of this work, which not only deals boldly with the legislative requirements of the colony in reference to the measures which are necessary for the preservation of public health, but also supplies an important gap in medical literature, that of a reliable handbook of the physicial features and medical peculiarities of the West African climate.

Dr. Horton has divided his treatise into two parts; in the first he treats generally of the physical conditions of the African climate, its seasons, and the meteorological features which they present; the second is purely medical in its nature, and deals with the diseases which prevail on different parts of the coast, and the circumstances which favour their production. An introductory chapter, which gives a brief outline of the various processes and instruments employed in taking meteorological observations, and their application to hygienic purposes, will be found useful to those who have not the more complete manuals of Dr. Parkes and Sir H. James at hand. A considerable portion of the first half of the book is devoted to a consideration of the subject of temperature, and a large number of observations taken by the author himself, at different parts of the coast, are recorded. Of these latter all that can be said is that they appear to have been carefully taken, but to very little purpose, as no conclusion is deduced by Dr. Horton from them, nor does one seem obvious. Indeed, if a brief summary of them had been given, which would have been quite sufficient for the objects of the present work, their details might have been omitted from it altogether, with manifest advantage both to its bulk and to the cost of printing it. Dr. Horton makes an interesting observation with reference to the influence which the high temperature that prevails during the greater part of the year in Senegal and Gambia exerts upon the physical and mental powers of the natives. The intense heat of the climate in which they live interferes, apparently, with the general nutrition of the body, and especially of the muscular system, their muscles being, as a rule, ill-developed, and their limbs attenuated and incapable of severe exertion, however short. The inordinate work which is thrown upon the excreting organs, also, often leads to gradual embarrassment of their functions, and to diseases in them which are unknown in more temperate climes. The result of this is that the duration of life is shortened, the average maximum of threescore and ten being seldom attained, and that old age sets in with premature rapidity, both of which effects are attributed by the natives themselves to the excessive heat. Nor is the influence of the climate upon the reproductive functions less marked; according to Dr. Horton, the virility of the males in the tropical regions on the coast is quickly exhausted, whilst the fecundity of the females is limited, a statement which, curiously enough, is stated to apply as well to the inferior animals as to man.

The most unsatisfactory portion of this section of Dr. Horton's work is that in which he deals with the effects of a continuous high temperature upon the nervous system. That the climatic conditions which accompany an elevated thermometer are not very favorable to intellectual any more than to physical exertion is a fact which is within the observation of every one, and which needs no elaborate speculations on the phenomena of the mind to explain it. But whether, as Dr. Horton argues, this is the sole or even the main reason of the inferiority in civilisation of the natives of tropical Africa to those of temperate regions is another matter altogether. No one can doubt that the mental culture of the young negro must be greatly deficient in that "sweetness and light" which we have been lately told should be the *beau ideal* of our own education, to say nothing of such subordinate subjects as the arts of reading, writing, and arithmetic; still less can there be any question "that," as our author somewhat pedantically puts it, "the fixation of their attention to certain objects, exclusive of many others" (a practice, which, by the way, the bonze fanatics of the East are said to cultivate to perfection), "is not made the constant study of their parents, who do not instruct them to call into their understandings the relation and connection which exists between different ideas, nor how to isolate ideas and recognise their common properties!" We should say that they would scarcely be negroes if they did, and we greatly fear that the young barbarians of the tropics are not much worse off in respect to this branch of their education than their pale-faced brethren in the north, whose "youthful consciousness" is more often "chastened" by physical appliances of a somewhat energetic kind, than by the mild and philosophic agencies by which Dr. Horton seems to imagine that they are trained. The gravity with which we are informed that one of the effects of a tropical climate is that "the automatic or attractive power after a short time becomes incapable, or finds it difficult, to rivet the attention on the class of ideas under consideration, and the thoughts, in consequence, either wander from the subject or do not develop themselves consecutively in the mind, and very often the individual complains of severe headache or vertigo," could scarcely be eclipsed by that with which the Bourgeois Gentilhomme's Maitre de Prosodie communicated to his pupil the startling information that he had been talking prose all his life. "In temperate climes," we are told with charming simplicity, "things are very different; there mental labour, apparently, is unattended by fatigue," the author having himself occupied fourteen hours out of the twenty-four in "hard study," and that "without any evil result," whilst in Africa six hours *per diem* is the limit of his intellectual exertions. If the inhabitants of Sierra Leone, of which colony Dr. Horton tells us in his preface that he is a native, are capable generally of performing similar exploits, we think that it is hardly fair in him to include them in the assertion that "the natives of tropical climates do not show the same active development of the intellectual faculties as those of temperate climates." The truth is, we take it, that if there be any inferiority in this respect on the part of the natives of the tropics, which in many cases there certainly is not, it is due much more to influences of race than to that of mere temperature. The philosophic literature of India and Arabia, the polity and civilisation of China, and the architectural remains of Cambodia, go a long way to demonstrate, even if more modern proofs were wanting, that a very considerable degree of intellectual activity may co-exist with a very high climatic temperature.

The most interesting and valuable portion of Dr. Horton's work is that which is devoted to a description of the topography and local conditions of the principal colonial towns on the West African coast.

It is impossible after reading it to come to any other conclusion than that the unhealthiness which attaches to them is mainly due to purely preventable causes, and that it might in many cases be almost, if not entirely, eradicated by energetic local improvement. When we read, for instance, that "the site of the British colony on the banks of the Gambia is so low that it is scarcely two feet above the level of the sea;" that "in the immediate neighbourhood of St. Mary's and at Combo there are extensive plains of offensive and pestilential swamps;" that M'Carthy's Island during the rains "forms an extensive plain of muddy swamp, with no high ground, with scarcely any drains to carry off the surface water;" that "in a sanitary point of view the site now occupied by the city of Freetown is the most unhealthy that could have been selected for the capital and *entrepôt* of trade," and so on of nearly all the towns on the coast; it is no subject for wonder that the ordinary mortality in them should be excessive, and that they should be periodically swept by hurricanes of pestilence which decimate the population thus predisposed to their attacks; nor that the average number of men in the regiments of the British army admitted into hospital during the year is 78 per cent., and the average number of deaths in the same period is 3 per cent. Dr. Horton thinks it strange that the European inhabitants whose lives are thus imperilled should not long ago have endeavoured strenuously to rouse the local government from its apathy on this point. So, perhaps, should we had we not abundant evidence at home that, with far less difficulties to contend with than beset the path of sanitary legislation in Western Africa, the same causes of mortality have been tolerated in our largest cities until a very recent date, and are even still to be found in many of our smaller towns, as well as in nearly all those of the Continent.

Of course, the remedies for this state of things are evident, and are strongly insisted upon by Dr. Horton. An organized system of sewage removal is the first necessity; but, strangely enough, whilst referring to this desideratum, Dr. Horton does not seem to be aware of the existence of Mr. Moule's dry-earth system, the results of which have been proved to be satisfactory, not only at home, but also in India and others of our tropical possessions. Then, in all towns which are liable to an accumulation of surface water, drainage in some shape is imperative. But, above all, a complete system of sanitary inspection and of the registration of mortality should be established. Without these the most elaborate sanitary legislation would prove but a dead letter, as we have found in too many cases from our experience in England. The recent consolidation of the local governments on the West Coast under a single central board will greatly facilitate the realisation of these objects, and the first step in the right direction has, we are glad to learn, been recently taken by the creation of a Board of Health at Sierra Leone. Let us hope that the labours of this board, under the scientific guidance of such advisers as Dr. Horton, will soon make the opprobrium of this colony, as the "grave of the white man," a thing of the past.

ART. VI.—*Transactions of the Obstetrical Society of London.* 1868. Vol. IX. Pp. 307.

JUDGING from this volume, the members of the Obstetrical Society have had much interesting matter brought before them at the meetings of which it is the record. Dr. Fairbank reports a case of "Fracture of the Pelvis in the Sixth Month of Pregnancy." The woman was crushed by a heavy waggon against a wall. Peritonitis and death of the fœtus followed, but uterine action was not set up until three months later, at the full period, when the contents of the uterus were naturally expelled, "the fœtus much wrinkled from long maceration, and the placenta very firm and consolidated." The woman became pregnant again some time after, and died in childbirth in consequence of the damage to the pelvis. Here we see the uterus exhibiting the ordinary phenomenon of exercising an expulsive action at a certain definite period, apart, apparently, from external cause. Why should not the uterus have retained its fœtus six months after the death and cessation of growth, whereas the date of labour at the three months corresponds to the ordinary puerperal cycle? What is the first step in "labour"? Dr. Barnes, in a paper on "Pregnancy complicated with Smallpox" (p. 102), suggests that "it is the influence of blood poor in oxygen and loaded with carbonic acid, in causing contraction of the involuntary muscles, exemplified in the aptness to abort in pregnant women suffering from asphyxia, chronic or acute, the same cause acting when, from any cause such as zymotic disease, the blood is rendered impure." May it be supposed that the fœtus arrived at a certain period of life so contaminates the maternal blood by the amount of excretive matter returned to the placenta that the uterine fibre is stimulated to contract? In cases of disease or death of the fœtus this may occur at an earlier period than nine months.

Dr. Hall Davis reports (p. 90) a case that occurred in Dr. Halley's practice, "Of Retention of Fœtus in Utero four years." Here the retained fœtus was probably pouched in the wall of the uterus, since a second pregnancy took place and went to the full term during the interval between the first pregnancy and the ejection of the fœtus piecemeal. Compare with this the case described (p. 75) by Dr. Braxton Hicks as "Intermural Fœtation," where death resulted from rupture of the uterus on the peritoneal surface and inter-abdominal hæmorrhage. The uterus (vide woodcut) presented an enlargement at the right side of its fundus, containing a placenta. The internal surface of this enlargement showed a ragged opening, as though the fœtus, having grown in the wall of the uterus, had finally burst into the cavity of the uterus, and abortion followed, rupture of the external wall of the uterus taking place four days later, consequent on contraction of the uterus to expel the placenta, which had not come away with the fœtus. Fœtal disease in uterô is exemplified by a case of peritonitis reported by Mr. Hunt, where the autopsy showed that the cause of the still-birth was acute inflammation of the peritoneum, caused by bands of old lymph strangulating the large intestine. Dr. Barrow reports a case of fœtal variola, and Mr. Curgenven fœtal convulsions of an hereditary character, and confined to the male infant.

Some contributions are made to the records of teratology. Dr. Ross details, with coloured drawings, a case of arrested development of the cranial bones. Dr. Eastlake, a case of complete epispadias, without extroversion of the bladder, the infant being also able to retain the urine. Dr. Meadows, a monstrosity, with a caudal appendage in the place of the lower extremities, the upper portion of the child being well formed, but the two iliac bones were fused into one, and connected to it by a sort of cup-and-ball joint was a single bone, a femur, having a patella on each condyle, and terminating by a small conical bone articulated with it by a hinge joint. Dr. Madge, on a case of spina bifida, with talipes varus, recalls the oft-noticed fact of impressions from external objects on the pregnant mother coinciding with monstrosity of the fœtus. Dr. Heywood Smith brings forward an interesting case of encephalocele.

The mechanical ingenuity of the obstetrician is represented by a variety of instruments. Dr. Matthews exhibited a new craniotomy forceps; Dr. Cleveland, a perforator; Mr. Mills, a speculum and a tenaculum; Dr. Meadows, an improved single-wire écraseur and a modification of the ordinary uterine sound; Dr. Sheraton, a new tire-tête or combined perforator and extractor; Dr. Eastlake, a drill crotchet. Carbolized sponge-tents are recommended by Mr. Ellis, while Mr. Aveling fills a hollow sponge-tent with permanganate of potash. We note with satisfaction that the old filthy tallow implement is being entirely superseded either by Sims' cleanly invention or at least by cacao butter. For full descriptions of these additions to the obstetric's armamentarium we must refer to the volume, in which the reader will also find an interesting paper, by Prof. Lazarewitch, on the induction of labour by injection of a *large* quantity of water towards the *fundus* of the uterus. The principle asserted is that uterine contraction normally commences at the fundus, and that, as this is more plentifully supplied with nerves than the cervix, a slighter amount of irritation than is necessary at the lower zone will bring about action, while by this process the bag of waters is unbroken.

Dr. Braxton Hicks, in a paper on obstructed labour, commonly called powerless labour, describes the condition of the uterus to be that of tonic spasm, and the exhaustion consequent on it as the result of reaction from the high state of local nervous excitement. He advises that ergot is in such cases contra-indicated, and manual interference should not be delayed.

The volume concludes with a paper by Dr. Snow Beck on a case of death after criminal abortion by puerperal septicæmia, and a discussion on the causes, treatment, and nomenclature of puerperal fever.

ART. VII.—*Arsenal de la Chirurgie contemporaine: description, mode d'emploi et appréçiation des appareils et instruments.* Par G. GAUJOT, Médecin-major, Professeur agrégé à l'Ecole Impériale de Médecine Militaire (Val-de-Grace), &c. Tome premier. Paris: Baillière et Fils. Pp. 772.

Armoury of Contemporary Surgery: a description of Apparatus and Instruments, with the mode of their Employment and an estimate of their Relative Value. By G GAUJOT, Surgeon-Major, Professor at the Imperial School of Military Medicine (Val-de-Grace), &c. Vol. I. Paris: Baillière and Son. Pp. 772.

THIS is the first volume of a work which purposes to give a complete account of the apparatus and instruments which are used in the practice of modern surgery. The subject which the author has chosen is a large one and not a very promising one, for the number of surgical instruments is constantly increasing, and the varieties of apparatus are constantly undergoing change, so that it is a difficult matter to give anything like a complete catalogue of either. Still, the undertaking is a useful one, and the author deserves no small credit for the way in which he has carried it out. The work will undoubtedly be of great service to the present generation of surgeons; but not for their sakes only has it been written. It is a record of the mechanical contrivances used in the practice of the healing art at the middle of the nineteenth century—a chronicle to which surgeons and mechanicians may hereafter refer to see what appliances have been tried in past years, and how far the discoveries of the future can improve upon them. In this way M. Gaujot's work may be a book of reference for many years to come, as well as a guide to the present use of instruments and mechanical appliances.

If we look into old books on surgery, or if we inspect collections of instruments, we may observe that very few new ideas have been struck out in the lapse of time. A catheter is still a catheter, a trocar is still a trocar—but how much improved in manufacture! The art of the mechanician has made great progress, and it is chiefly through the improvement of his materials and the ingenuity of his contrivances that surgical instruments have been advanced to their present state of perfection. If we wished an example of the truth of what we have been saying, we should point to the manufacture of lithotrites. How perfect an instrument, both in material and in contrivance, is the modern lithotrite compared with those which were devised long ago—even by Abulcasis—for crushing stones in the bladder. Nor is there any reason why we should suppose that mechanical skill or ingenuity have reached their limit. The future will probably show as much progress as the past—perhaps a much more rapid progress—and it will be by reference to such works as the one before us that posterity will be able to measure the degree of their advancement.

Only the first volume of M. Gaujot's work has come into our hands as yet. In it he treats of a variety of subjects, including all those which have to do with the performance of operations and the treatment of injuries or orthopædic cases. This is a wide field, and embraces a vast number of the most important surgical appliances; and our author has shown great industry in the way in which he has fulfilled his task. He has extended his studies over a large area of European and American literature, and he has brought together almost every contrivance of real value. Some of those which he has inserted hardly deserve a place in his pages. They are obsolete and out of use, and have little more than an antiquarian interest. Such contrivances might very well have been omitted and allowed to slumber in the pages of bygone books—to be resuscitated in due time, if this should ever seem to be desirable; but to introduce them into a modern work, and to increase the size of a volume already quite large enough, seems hardly calculated to serve any good purpose.

A more serious fault, however, than the introduction of obsolete appliances is the omission of some of those which are the simplest and the most common. Perhaps they have been passed over because of their very simplicity; perhaps it was thought that they hardly rose to the dignity of apparatus. However this may have been, their handiness and their frequent use in practice ought to have insured them a place. Thus we see that our author gives various ways of applying irrigation, but he omits the simplest of all—namely, by putting a few strips of lint into a bottle of water, and suspending it over the part. Again, in speaking of transverse fracture of the patella, he enumerates several modes of treatment, but he does not mention the common way of bringing the fragments into apposition by a figure-of-eight bandage secured over a notched splint or over a splint furnished with hooks on its under surface. It would be easy for us to mention other examples of the same thing, but these are enough to show what we mean when we say that the author has omitted some of the simplest and most handy appliances, while he has taken great pains to bring together elaborate and complicated ones.

The diligence which M. Gaujot has shown in the performance of a difficult task, and the wide extent of his reading and research, deserve great praise, and we do not doubt that he will have his reward. If we mistake not, his work will long stand as a book of reference to which the surgeon may turn in difficult and unusual cases, when he wishes to see what aid he may hope to receive from mechanical appliances.

ART. VIII.—*Oppolzer's Vorlesungen über specielle Pathologie und Therapie.* Bearbeitet und herausgegeben von Dr. EMIL RITTER VON STOFFELLA. Lieferung I, II, u. III. Erlangen, 1867-68.

Oppolzer's Lectures on Special Pathology and Therapeutics. Prepared and edited by Dr. STOFFELLA. Vol. I; Parts I, II, and III. 8vo, pp. 534.

To no country is medicine, in recent times, more indebted for successful progress than to Germany. Physiology and minute anatomy and physiological chemistry, as there cultivated, have been converted to English use largely, by translation of works as soon as published, and even still more largely by being employed as the basis of most of our best reputed treatises on those subjects. Indeed, it may be objected that it has grown too much the custom to import facts in those sciences from Germany to the detriment of English research. Moreover, by the writings of Virchow especially, among several others, German pathology bids fair to secure as high a position in the estimation of our countrymen as of old German physiology has done. At the present time we have to make known the appearance of a book on practical medicine which cannot fail to raise our estimate of German physicians as clinical teachers.

Dr. Stoffella has done well to edit this collection of lectures. As a former pupil, and for several years clinical assistant of Oppolzer, he is well fitted for the task he has undertaken. Prof. Oppolzer's name is not so well known in England as it should be; but in Vienna no professor is more popular, and he holds there, as a clinical teacher, a similar position to that enjoyed by the late lamented Trousseau in Paris. The lectures are published with his approbation, and his old pupils will heartily welcome their appearance.

As yet three parts are produced, containing above 500 pages devoted to the consideration of diseases of the heart and great blood-vessels, and of the respiratory organs. From the space allotted to these subjects, ample scope has been given for their full discussion. Being prepared as lectures, the style is particularly easy and clear. Without introductory observations or preface, the treatise at once commences with a chapter on pericarditis. This is so subdivided as to present, in succession, a general sketch or definition of the disease, an account of its etiology, of its pathological anatomy, of its symptoms and course, and of its diagnosis, prognosis, and treatment. The same subdivision of the subject is followed in the consideration of the other diseases brought under notice, the sections on symptoms and diagnosis being in every instance particularly complete.

The instruction imparted bears throughout the mark of a physician of wide experience, of accurate powers of observation, of superior sagacity and conscientiousness, and gifted with the enviable faculty of clearly expounding the knowledge he has acquired.

No prospectus is affixed to either of the parts now published, to advertise us of the extent of the work when completed; but, judging from the subjects already handled, and the space they have taken up, it is quite clear that if the whole list of medical diseases is to be dealt with on the same scale, the treatise will form at least three volumes octavo, each of at least 500 pages. The extent of the work, therefore, calls for the speedy issue of the several parts—a requirement that, unfortunately, does not seem to have occurred to the mind of the editor, who has succeeded in passing through the press only one part in the course of each year, the three parts now before us bearing date severally 1866, 1867, and 1868. At this rate of progress the publication of the lectures will occupy the remainder of the lifetime of many readers, and will assuredly be destructive of the commercial success of the work. If these remarks meet the eye of the worthy editor, we trust they will stimulate him to activity. Were the lectures his own composition, a shadow of excuse might be made for the slowness of their publication; but when it is considered that he has simply to pass matter already prepared through the press, no allowance can be made for his dilatory progress.

ART. IX.—*Contributions to the Study of some Thoracic Diseases.* By JAMES CUMING, M.A., M.D. Dublin, 1868. Pp. 22.

THIS pamphlet is a reprint of some clinical papers on thoracic diseases that appeared in our contemporary, 'The Dublin Quarterly Journal of Medical Science,' in February and May, 1868. The first paper refers to the occurrence of mitral regurgitation not arising from organic disease; the second is occupied with the record of a case of aneurysm of the thoracic aorta, in which tracheotomy was performed to relieve impending suffocation dependent on the narrowing of the trachea by the pressure of the tumour, and on stretching of the recurrent laryngeal nerve. This case is illustrated by an engraving showing the aorta, the aneurysm, and the parts implicated by the latter. The last communication is of a case of empyema, with inequality of the radial pulses and paralysis of the diaphragm.

Those engaged specially with the study of thoracic diseases will be pleased to obtain these illustrations of less common lesions, particularly when derived from so successful an observer as Dr. Cuming.

ART. X.—*A Manual for the Classification, Training, and Education of the Feeble-minded, Imbecile, and Idiotic.* By P. MARTIN DUNCAN, M.B., &c., and WILLIAM MILLARD. London. Pp. 191.

THE efforts which are now making to establish asylums for idiots, of which the northern counties are showing a noble example, afford the best proof of the increased interest taken in these hapless beings, and of the earnest desire generally felt to alleviate their condition.

The work before us is well timed, and is in every way creditable to its authors. Its comprehensive title-page is sufficiently indicative of its design. In the preface they modestly state that they do not propose to give a scientific history of idiocy, the difficulties of which they seem fully to appreciate, but to limit themselves to what is practical, resting their qualifications for the task on a long acquaintance with the peculiarities of those unfortunate children who cannot be educated by ordinary methods.

The classification which they propose of the varieties of idiocy seems well adapted for the ends in view. As very informing in itself, we cannot do better than quote it with the remarks accompanying it:—

IDIOTS, CONGENITAL.	CLASS 1.—True and profound idiots. Solitaries.	The affliction exists at birth, and there are malformations and disabilities of the parts of the body, as well as deficiencies of the mental power. They may be described as beings suffering from various functional disturbances, from perversion of special and common sensation, from paralysis of sets of muscles, and from inability to co-ordinate many more or less complicated muscular movements to a common end.
	CLASS 2.—Having a slight amount of intelligence; being able to stand and walk a little, and often capable of slight instruction.	
	CLASS 3.—Able to walk, run, use their fingers; to be made to attend slightly to do easy mechanical work, and to feed themselves; memory and perception very weak, and variable in power.	Their walking is defective in ease and grace; the voice is rarely or never perfect; the memory, intellectual perception, foresight, &c., are very defective, and often absent. The power of abstract reasoning is generally wanting, and seldom to be traced, except in a very slight degree. Automatic movements are common.
	CLASS 4.—Feeble-minded children, adolescents, and adults.	Cases higher than Class 3 in the scale of mental and physical power, and reaching to the lowest class of intelligence found in perfect individuals. Many are born in the Class 1, and by growth and training emerge into this class. They have all more or less defective voices and power of locomotion; a great want of foresight, common-sense, and powers of self-management.
IMBECILES,[1] NON-CONGENITAL.	CLASS 5 . . .	Born with perfect intelligence, and with the usual gifts of children. A state of mind like that of the classes already noticed has been produced by disease of the brain, by epileptic convulsions, by water on the brain, or by injury of the head. Malformation may or may not exist, and the muscular powers, with the ability to walk and speak, are very variable in extent.
	CLASS 6 . . .	Resembling Class 5, but the evidence of permanent disease of the brain exists in the form of epileptic seizure and paralysis.
	CLASS 7 . . .	Cases born with hydrocephalus, or in which the disease has been arrested after it has destroyed, more or less, the power of the brain,
	CLASS 8 . . .	Cases of perfect individuals who have been educated, and who have become debased in mind and body during early youth from vice.

This classification is followed by illustrative cases. It would be futile to attempt an analysis of the eighteen chapters in which the various matters concerning the education of idiots are discussed, and, we may add, carefully, minutely, and judiciously.

It is some satisfaction to have the assurance of the authors, founded on their experience, that even in the worst cases there is a scintilla of intelligence, and that even in these cases a certain degree of improvement may be effected by unremitting attention in the employment of the most efficient means.

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[1] "The subject of cretinism is not considered, because such cases are not common in the United Kingdom."

It is said, and if we are not mistaken by the authors, that idiocy is found to increase with increase of civilisation. The return given in the appendix of the number and proportion of the pauper-idiots in England and Wales does not appear to support this opinion. Thus, in a total population of 19,962,641, there are 10,021 idiots, averaging one in 1992. Now, referring to the several counties, what do we see? Not a larger, but a smaller proportion in those in which, it may be presumed, civilisation and education are most advanced; for instance, whilst in North Wales the proportion is one in 906, the greatest of all; in Middlesex it is lowest of all, one in 5037.

Art. XI.—*Annali delle Epidemie occorse in Italia dalle prime Memorie fino al* 1850. Scritti dal Prof. Cav. Alfonso Corradi. Parte I et II. 4to. Pp. 364 and 352. Bologna, 1865 and 1867.

Annals of Epidemics which have occurred in Italy from the earliest Times down to 1850.

This is a work of the highest merit, displaying great erudition, immense research, and an ardent love of the subject treated therein. It cannot fail to prove most interesting to the historical student as well as to the medical philosopher, from the records given not only of all the principal epidemic diseases which have appeared in Italy, but also of the associated epizoötics and epiplytics, famines, volcanic eruptions, and eclipses.

Part I commences with a notice of a pestilence in Rome in the eighth century B.C., mentioned by Plutarch, and it brings down the narrative to the end of the fifteenth century of our era.

Part II is entirely occupied with the events of the sixteenth century, during which many remarkable outbreaks of epidemic disease took place in different regions of the Italian peninsula, and in other countries of Europe. The true or oriental plague was then, perhaps, the most destructive scourge which desolated the land, although it is difficult often to distinguish its outbreaks from those of the malignant or petechial typhus, which seems to have been seldom absent, as a fatal epidemic, for longer than four or five years at a time. The tabulated index, at the end of this second part, of all the leading events hitherto recorded, will be found most useful for reference.

A promise is given that the subsequent parts of the work will ere long appear. The interest of these will, of course, increase as the record comes down to more recent times.

Dr. Corradi announces his intention, upon the completion of his present labours, to write a 'History of Public Hygiene in Italy.' Such a work would assuredly be of the highest importance nationally as well as professionally; for there is probably no country in Europe where a larger amount of preventable disease and of unnecessary mortality is at all times, and especially in epidemic seasons, going on than in the fair land of Italy, in consequence of general apathy and ignorance.

Cordially wishing him every encouragement and success in his public-spirited enterprises, we would strongly commend these annals to the attention of all who are interested in following the chronology and geographical distribution of epidemic diseases. The work is inscribed to Dr. Daremberg, of Paris.

ART. XII.—*On Chronic Bronchitis, especially as connected with Gout, Emphysema, and Diseases of the Heart.* Being Clinical Lectures delivered at the Middlesex Hospital by E. HEADLAM GREENHOW, M.D., F.R.C.P., Senior Assistant Physician to the Middlesex Hospital. London, 1869. Pp. 236, post 8vo.

WE have often lamented the waste of material for scientific investigation and teaching which occurs amongst the out-patients of our hospitals and dispensaries. The medical officers who take charge of them fall into routine habits of prescribing, not so much from carelessness or sluggishness, as from sheer despair at making these monotonous crowds of minor ailments interesting to themselves or edifying to the student. It never has been done, they think, and they do not see their way to doing it; so they get through their duties to the patients in the readiest way they can, and bide the time when a charge of in-patients shall give them an opportunity of showing that they really care to advance clinical knowledge. We therefore hail with peculiar pleasure a complete answer to this despair, by which it is left without excuse, in the shape of a volume of clinical lectures, given by an assistant physician, on a class of patients of whom not one out of ten is suited for admission to the in-wards, except in the most temporary fashion. We grant that the task of working up the waste material is not an easy one, still less is it easy to make such a good use of it as Dr. Greenhow has done. But these positions are usually held at a time of life when the consciousness of energy makes labour sweet and self-rewarding; and those who occupy them must remember that possibly the opportunities of usefulness to which they look forward may not arrive till the time and desire to seize them has passed away. We do not expect to see many such an agreeable book as Dr. Greenhow's, but we will accept with gratitude even unequal contributions to literature.

One great value of the collection of cases here placed before us, and, we presume, originally brought directly under the eyes of the pupils, lies in their everyday character. The reader fancies himself in the greasy old familiar chair at the dispensary or out-patients' consulting-room, listening to the oft-repeated, yet ever various, tale, day after day. By which day-dream we hold we are paying the highest possible compliment to the literary execution of the work, and to the capabilities of the author as a teacher of men. Let not the admirer of novel, sensational, and exceptional cases expect a find in these covers. They are, to our thinking, judiciously blank in respect to this sort of game. They bear to such literature much the same proportion that Anthony Trollope bears to Mrs. Radcliffe, or 'Pepys' Diary' to Caulfield's 'Remarkable Characters.' But in vivid pictures of the sort of cases which a practitioner encounters in his daily walk, and an example of the way in which a student ought to turn them over in his mind and make them tools for self-improvement, we have rarely seen a volume richer.

Any sort of originality Dr. Greenhow almost captiously disclaims. For example, in tracing the connection between bronchitis and gout, he quotes Sir Henry Holland and other authors as having hinted at it; and thus some readers might fail to observe that he is really the first who has by clinical examples told us what is implied in the connection, and, moreover, told us what he individually means by "gout" in such instances. By that much misused word he means such evidence of the existence of an excess of uric acid in the blood as is afforded, not only by intermittent attacks of the specific inflammations of the smaller joints, but also by chronic arthritis and a tendency to psoriasis and eczema. Perhaps he admits somewhat too easily the testimony afforded by uneducated patients to the development of these diseases in their ancestors and kindred, but at all events he relates frankly the source of his information, and is freer from mysterious statements of his experience than most medical writers. He certainly makes out the point of an abnormal prevalence of these conditions among bronchitic patients. But his most telling cases are those where the ailments alternate, where the bronchitis seems to cure the gout and the gout the bronchitis. The old-fashioned use of colchicum and iodide of potassium for chronic coughs is thus explained and rendered rational.

In the pathology of emphysema, again, he thinks it needful to relate the varying opinions of many respected physicians, who have assigned as the immediate cause of the dilatation of the air-cells, now inspiration and now expiration, concluding rightly by the attribution of the dilating force of the latter. But he might have cut the matter much shorter by citing the experiments of Mr. Hutchinson, which, some twenty years ago, set the matter at rest by exhibiting the greatly superior force of expiratory efforts, and illustrating the point with the reminder that rotten bellows are burst, not in drawing the air in, but in driving it out.

In discussing the bronchitis induced by mechanical irritation, Dr. Greenhow draws attention to many more sources of that irritation than are usually reckoned upon. Besides the instances of stone-masons, dry metal grinders, mill-stone makers, and other inhalers of heavy particles, which are considered typical, he calls attention to the case of light-dust-breathers, such as wool-workers, feather dealers, paper stainers, and especially chaff-cutters. He suggests also, as a frequent analogous cause of bronchitis, the inspiration of air over-dried, and, therefore, loaded with irritating vapours and powdered organic matter. This particularly applies to the products of combustion of gas and coke, and to the unventilated workshops of tailors and milliners. The author here digresses a little from direct clinical illustration to denounce chimney corners, closed windows, padded doors, and temperatures regulated by the thermometer, on which persons with "delicate chests" are apt to rely for a prolongation of life:

> "By far the greater number of bronchitic patients do better when not kept in rooms at a high temperature, inasmuch as they sustain less injury to their general health, and are able to go about earlier, and with less risk of taking cold from any slight exposure, than those who have been so confined. It is not so much a warm atmosphere that is needed by bronchitic patients as ample protection from chilling from the surface, and this may be secured by suitable clothing. In fact, as a rule, I have found that, other things being equal, chronic bronchitis is slower in its progress, and less speedily affects the general health, in persons whose duties take them much out of doors, than those who from their indoor occupations would generally be presumed to be less exposed to causes of taking cold, and therefore less liable to accessions of their complaint."

The importance of not confining our therapeutic attentions to the part most obviously affected is a prominent feature in all the best modern hygiene, as distinguished from the local medication of twenty years ago. In pulmonary complaints, especially, each new work insists more and more strongly that (as Dr. Chambers says) "the chest is the battle-field of past conflict, the stomach the ripening ground for new levies of life."

In respect of the connection between cardiac lesions and bronchitis, the point which Dr. Greenhow endeavours to make out is that where the heart disease causes the pulmonary affection it is of the left side of the organ; whereas when the bronchial incompetency is the original ailment, and the cardiac the secondary, the right portion is disorganized. We believe the same rule is applicable to the connection between injuries to the circulating organs and those of any other viscus. The most serious injury, that of the left side, is primary, that of the right side secondary. We exclude, of course, congenital malformations.

ART. XIII.—*Undersögelser angaaende Barselfeberen i Danmark udenfor Kjöbenhavn.* Afhandling for Doctorgraden i Medicinen, af G. G. STAGE, Candidat ved den kgl. Födselsstiftelse. Kjöbenhavn: C. A. Reitzel. 1868. 8vo. Pp. 136.

Researches on Puerperal Fever in Denmark, excluding Copenhagen. A Thesis for the Degree of Doctor in Medicine, by G. G. STAGE, Candidate at the Royal Lying-in Institution. Copenhagen: C. A. Reitzel.

THE author's object in the above essay is to give a sketch of the puerperal fever as it appeared in Denmark, with the exception of Copenhagen, during the twenty-three years 1844—1866, both inclusive. He selects this period because in 1844 tables of mortality for the whole country were introduced, which he wished to use, and also because in the same year a great epidemic of puerperal fever occurred in Aalborg, which he was desirous of including in his description. The material, however, from which he chiefly draws his statistics is supplied by the yearly reports sent in to the College of Health by every physician throughout the entire country.

Candidate Stage quotes the statements of numerous district physicians to show that, excluding Copenhagen, puerperal fever is, on the whole, a rare disease in Denmark. He appends tables of the mortality in childbirth in all the market towns in the kingdom during the ten years 1857-66, from which it appears that they amounted to 323 deaths among 70,735 parturient women, or 1 in 219, or 0·45 per cent., and he then proceeds to inquire into the distribution of puerperal fever in the different years, and in the different parts of the country; into the relation between its extension in the towns and in the rural districts; and, lastly, into the mode in which it occurs, whether sporadically or epidemically. With respect to the first point, it appears from the lists of mortality in the market towns that the proportion of deaths to deliveries in 1861 was 1 in 389, in 1862 and 1863 the mortality was below the average, while in 1864 it suddenly increased very considerably, amounting to 1 in 107, being thus more than trebled. In the following year the mortality again fell considerably. The tabular lists from the rural districts showed that the number of sick was greatest also in 1864.

As to the distribution and comparative malignity of the disease in the towns and rural districts, the author is unfortunately unable to give positive information in consequence of a defect in the yearly reports and lists sent in, in which no distinction is made between town and country. Hr. Stage, however, states his belief that puerperal fever in its malignant form is rare in the country, the physicians often indicating a difference between country and town in favour of the former. So far as he can draw any conclusion from a comparison between the lists of mortality for the towns and the statistics collected by himself, the difference in mortality between town and country would seem to be diminishing.

The author next devotes a brief space to the consideration of the *forms* under which puerperal fever occurs, premising—

> "That there is a very great difference between the circumstances in an hospital, where the patient is examined in a clean bed, cleanly clad, with the opportunity of examining her as often as may be necessary and with all aids at hand, and those under which patients are found in the more common part of country practice. That very precise diagnoses are often impossible under these latter conditions is a natural consequence; and, moreover, post-mortem examinations are, in such cases, rarely made by physicians in the country."

Of the fatal cases, incomparably the most frequent are those of metroperitonitis. Metrophlebitis, perimetritis and oöphoritis, are very usually mentioned, as well as the milder cases of metritis and endometritis. Of 148 fatal cases of puerperal fever, on the reports of which the author has special reason to rely, the disease was in 71 instances stated to be "metroperitonitis."

The author introduces his observations on the *causes* of puerperal fever in the following words:

> "Considered from a practical point of view, the interest which attaches to the study of the causal relations of a disease will depend essentially on the frequency and malignity of the latter, circumstances which together decide its importance to society. In this respect everything calls on us to institute accurate investigations as to the origin of puerperal fever. For although, as we have seen, this disease does not in its malignant form occur frequently, either in the country or in the towns outside the precincts of the lying-in hospitals, it acquires so much the greater importance for the latter, those most humane institutions of the present day, and this to such a degree that we have sometimes been obliged to take into serious consideration whether the dangers to which we expose both parturient women and their children, by taking them into the institutions, do not more than counterbalance the advantages these afford. And even outside the institutions puerperal fever, in its epidemic diffusions, may be destructive enough. Another important reason for investigating the causes of puerperal fever is, that it is one of those diseases whose course we can, unfortunately, only in a very slight degree influence, as it most frequently terminates in death when it

has once been developed in its malignant form. The most important part of our interference must be directed to prevent the disease, in which we can be successful only through an accurate study of its cause, which is, again, most intimately connected with the question of the nature of the disease."—P. 49.

Hr. Stage proceeds to consider the *causes* of puerperal fever as they relate to its *sporadic* and its *epidemic* form, separately. Among the most important established sources of the sporadic variety are *protracted and difficult labours*. This the author illustrates by accurate tables, and the fact is further borne out by the reports of five physicians, embracing 6111 deliveries, in 28 of which the mothers died. Of these 28, seven were delivered by the forceps, and six by turning, a large proportion.

"That *putrid abortions, remains of the membranes and coagula of blood* retained in the uterus, and undergoing decomposition, may generate puerperal fever, is well known; and of this examples are not wanting in our reports.

"Further, we may, as Winckel[1] remarks, mention *primary catarrhal inflammations* of the vagina and uterus, as they in the course of the *accouchement* may be developed into acute parenchymatous inflammations; under the influence of the catarrhal secretion the minor lesions occurring during delivery may be converted into ulcers, which become irritated, discoloured, and may thus form the nidus of more profound inflammations. The explanation of almost all these cases is, therefore, this, that the individual herself, in some mode or other, develops putrid matters, which are absorbed and produce an infection—the so-called *self-infection*—in contrast to an infection from without. These different causal elements, together with an infection from without, which I shall presently speak of, are, in fact, so generally diffused, that, as Winckel remarks, in a given case we shall not easily be in a position to exclude them all."—P. 58.

We may here remark that Dr. Evory Kennedy, formerly Master of the Dublin Lying-in Hospital, has recently propounded a theory which promises to throw light upon the development, not only of puerperal fever, but of zymotic diseases in general, and to assign to each agent its proper part in the production of the same.

"The local cause," he observes, "of puerperal fever approaches more nearly to a constant quantity in the wards of a lying-in hospital, whereas it is only an occasional quantity in the houses of the affluent, and the sole influence exercised in its production by the atmosphere is that, in certain states of the latter, the constant and occasional quantities become more operative or active in generating and propagating this dreadful malady—a malady zymotic in its type and origin, produced by a poison emanating from parturient women, more active in proportion to the concentration of their excretions or exhalations, and consequently in proportion to their number cohabiting in a given atmospheric space, but not requiring more than one parturient female to generate it when the poison she herself has developed may, as in the case of blood-poisoning, be reabsorbed into her own system, and self-contamination then as certainly strike her down as if a crowded, ill-ventilated lying-in ward were the generating medium."[1]

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[1] 'Die Pathologie und Therapie des Wochenbetts,' Berlin, 1866, p. 259.

In addition to the foregoing, Hr. Stage speaks of *individual peculiarities*, psychical and bodily affections, errors in diet and such like —in a word, the patient's whole condition and mode of life before and during delivery.

The author next proceeds to consider the causes of puerperal fever in its *epidemic* form, to which he refers cases occurring in unusual number, whether they proceed from properly "epidemic"—that is, from atmospheric or telluric—causes, or from direct contagion. "The investigations of recent times lead us, in general," he observes, "more and more to the conviction, that every disease, which in the epidemic form can be conveyed from place to place, is also contagious." And a little further on he adds:

> "We have just seen that several different causal elements, each of which is in itself capable of generating puerperal fever, exist, either collectively or singly, in almost every parturient woman, so that scarcely in any case can we exclude them all; we cannot, therefore, be surprised to see the disease occur, as it in general does, in some scattered cases, and such will scarcely anywhere be wanting. But how do the epidemic propagations of the disease occur? This is both the most important and, at the same, the most difficult point in the etiological relations of puerperal fever."—P. 64.

That the disease is communicable by contagion cannot for a moment be doubted, but many, while they admit this, deny the existence of a specific miasma. There is scarcely any one, on the other hand, who will discard altogether the importance of general atmospheric influences—heat, cold, and moisture—with respect to puerperal fever. The author appends many lamentable examples of its dissemination by contagion. He enters at some length into the consideration of the relations of puerperal fever to ague, scarlatina, erysipelas, and diphtheria, appends numerous reports from country practitioners bearing chiefly on the contagiousness of the disease, and concludes his useful and practical little volume with a chapter of comparative remarks respecting puerperal and "calving" fever, a very similar affection occurring, though only very rarely, in the cow.

ART. XIV.—*Croonian Lectures on Matter and Force.* By HENRY BENCE JONES, A.M., M.D., F.R.S., Consulting Physician to St. George's Hospital. Pp. 223.

IN the important work on pathology and therapeutics, by the same author, which we reviewed last year, occurred the following passage:—"We have ceased to look on the human machine as a creator of vital or nervous force, and we are beginning to regard it as a converter of latent into active force." The sentence points in the same direction as the present work, and might almost have been taken as its text, for it comprehends, and to a great extent may be said to consist of, a vigorous attack on the yet lingering belief in a separable vital force.

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[1] 'Medical Times and Gazette,' vol. i, 1869, p. 23.

The plan of the work is somewhat singular. It consists of the three Croonian lectures, containing the history of our ideas respecting the relations of ponderable matter and of force, followed by a brief, well-reasoned, and tersely written argument for the inseparability of vital force from the matter in which it has been placed. All this portion is, of course, by Dr. Jones himself; but it is followed by three appendices, considerably more voluminous than the lectures to which they are attached, and each of great interest.

The first of these, "on the time required for the transmission of volition and sensation through the nerves," is by Du Bois-Reymond; the second, "on the source of muscular power," by Dr. Frankland; and the third, "on the rate of passage of crystalloïds into and out of the vascular and non-vascular textures of the body," is a reprint of the author's own valuable paper in the 'Philosophical Transactions.'

What may be called the historical portion of the work is again divided into two sections, the former of these relating to "sciences into which the idea of life does not enter," or, as Dr. Bence Jones elsewhere calls them, "the abiological sciences." The word is cumbrous, but saves a periphrasis which would otherwise be frequently occurring. To us the most interesting part of this chapter is the history of the development and decay of the ideas founded on the hypothesis of the inseparable union of force with imponderable matter. These ideas, of which the first traces seem to begin with Kepler's vortices, ruled the scientific world during the whole earlier portion of the modern period; and though they are now rapidly giving way and disappearing, it must be allowed that, however unsound their basis, they served to build up a great fabric of physical science, which has not, to any great extent, crumbled away with the undermining of its foundations, but still stands coherent as a platform for present and future builders. It was in the department of light that the modern doctrine of the inseparability of force from ponderable matter first began to sap the foundations of the structure, but even here some advocates of the older views survive, and decline to quit the lines on which Newton took his stand. At page 24 is quoted from Grove a short and clear statement of the position of the controversy between these philosophers and the newer school. "To account for the phenomena," says he, "the æther is assumed, and to prove the existence of the æther the phenomena are cited. For these reasons, and others above given, I think the assumption of the universality of ordinary matter is the least gratuitous."

The department of heat was next invaded by the new theory, and here the changes are marked pretty clearly by the alterations in terminology. The successive employment of the words phlogiston and caloric marks successive stages of thought as to the nature of phenomena, and even the latter word has now almost disappeared from our books. The phenomena of electricity furnished ground that long continued to rise high above the advancing flood. Faraday even, in 1821, wrote favorably as to the materiality of electricity, though he allowed that it was only a supposition, but in his latter days he powerfully upheld the newer doctrine, in connection with that of the conservation of energy.

Dr. Bence Jones sums up the present state of our ideas respecting the union of matter and force, where there is no question of life, in the following words:—" Where matter is, there force must be, showing itself in motion, or in tension, or in resistance. Without matter, no kind of motion, nor tension, nor resistance, occurs."

He then proceeds to examine the question whether the biological sciences will, in this aspect, follow in the footsteps and take the complexion of all other sciences; and first, whether they have hitherto from their very origin done so, and whether the stages of their development may be compared with those of the abiological sciences. In no country has the popular idea, at all events, yet proceeded beyond the author's first stage, that of the complete separability of matter and force, *i.e.* of corporeal substance and vital force.

> " The popular idea is that no distinction exists between the life, the mind, and the soul of man; all are confused together, and all are thought to compose a single immaterial spirit, which comes at birth and goes at death, being perfectly separable from the matter of which we are made."

Dr. Jones's second historical stage of ideas, that of partial separation in the ideas of living force and matter, "is marked," he says, "by the assumption of a living imponderable gas or fluid pervading the ponderable matter" of the animal body. This view was certainly held by Hunter and others, and a nervous fluid akin to, or possibly identical with, electricity, was for some time a favorite conception. It is at this point that Du Bois-Reymond's appendix, already mentioned, is introduced, in order mainly, it would appear, to show that the ordinary rate of transmission through a nerve of that "motion which gives rise to sensation or to (muscular?) motion, is only between twenty-eight and thirty-three feet in a second, and thus to point out the improbability of its being a form of electricity."

Du Bois-Reymond's lecture is an excellent *résumé* of the state of knowledge on his subject, full of interesting facts, some of which may, perhaps, one day come to have a practical bearing on medicine. The velocity of nervous force is estimated as less than that of the flight of the eagle. But perhaps the most striking illustration is the following:—Supposing a very large whale to have its tail struck by a harpoon, it will take about a second before the pain reaches the animal's brain, and, neglecting any time that may be required for the processes that may follow in the brain, "nearly another second must elapse before the order can be telegraphed to the muscles of the tail to capsize the boat."

To return. The author speaks of a vital fluid which was supposed, or is still supposed, "to impart to the constituent matters of the animal frame new chemical affinities between it and the surrounding elements, and thereby to protect the living fibre from dissolution, or to give it other properties which, without this VITALISATION, it would not possess." We think the holders of this view did not, or do not, usually conceive of the vitalising agent as a fluid or imponderable matter. Most of them are, we think, in the first rather than in the second of Dr. Bence Jones's stages of thought. The author proceeds to demolish the idea of a vital action in the coagulation of the blood, and then passes on, *àpropos* of Dr. Prout's views as to the production of urea in cases of luxus-consumption, through defect of vitalisation, to draw from Kühne's experiments on the action of pancreatic juice the rudiments of a theory as to such production, which, of course, dispenses with any kind of aid from a vital force.

We now come to the third lecture, the crown of the work, to which, indeed, the first and second, being almost wholly historical, are introductory.

The author begins by stating, with great moderation, the argument from analogy, the probability that what is true in other departments of natural science is true in the region of biology, seeing that the kinds of matter concerned are not peculiar in their essence, and are capable of being acted on by the same varieties of force.

> "The unity of nature," he says, "will at least be preserved by our hesitation to admit the assumption of a force capable of creation and annihilation, until some very conclusive evidence is obtained that there actually is in living things such a force capable of being separated entirely from the matter of which they are made."

Next, he endeavours to point out some of the directions in which the recognition of the applicability to biology of the views already expounded is likely to produce speedy and valuable fruit. He applies them accordingly to the problem of the source of animal heat, and to that other, so nearly allied, of the origin and maintenance of muscular motion. The second appendix, consisting of Professor Frankland's paper, "on the source of muscular power," may be regarded as an overgrown note to this part of the work; but we use the word overgrown in no derogatory sense, for the paper is an admirable *résumé* of the subject, carried down to a point subsequent to Playfair's and Haughton's, but anterior to Parkes' publication upon it. It contains full and clear details of the celebrated Faulhorn experiment of Fick and Wislicenius, whose views form the groundwork of those of Frankland himself.

The author next proceeds to confront the irrepressible problem of the genesis of inflammation. We think him scarcely so happy here as in other parts of his work, not being able to see how the principle of the conservation of energy can be directly and necessarily opposed to the theory of the production of inflammation through paralysis of the contractile textures of the small vessels.

Most of the remainder of the lecture is occupied with a discussion of the probable effects on therapeutics of the modern views respecting force or motion. For numerous details on this subject the reader might refer with advantage to the larger volume of the author on pathology and therapeutics, but some of the generalisations brought out in the present work are of the highest interest and importance. Take, for example, the following:

> "This view will almost lead us to consider all medicines as alteratives; and if so we may, perhaps, place stimulant and sedative medicines at the two extremes of the alterative actions, the stimulants giving rise to the greatest increase of motion, and the sedatives allowing the least motion or the nearest approach to rest. Hence rest and motion may be taken as the two great aims of therapeutical actions."

The weakest point of this part of the case, the author seems to think, is that where he has to oppose the theory of the elective local affinity of medicines. Here accordingly he strengthens himself by detailing his (and Dr. Dupré's) elaborate experiments on the passage of lithium, quinine, and other substances, into and out of the animal body, which form the material of the third appendix. The whole inquiry is of very great interest and pregnancy, but its results are certainly insufficient to disprove the theory above alluded to, until confirmed by extension to a number of other substances.

Finally, from these details the author expands again into the full breadth of his leading idea, and, confronting the prejudices of those who are ever ready to use the word materialism in a reproachful sense, takes his leave of the reader in the following words:

> "Our ideas of the grandeur, the unity, and the power of the first

cause, will surely not be lessened if it can be shown that one law of the union of force and matter and of the conservation of energy obtains throughout the organic as well as throughout the inorganic creation."

ART. XV.—*Journal of the Scottish Meteorological Society.* New Series, Nos. XVI, XVII, XVIII, XIX, XX. October, 1867; January 1868; and Oct., 1868. Pp. 64. Quarterly. Edinburgh and London.

SINCE the days of Herodotus, whose quaint explanation of the yearly passage of the sun north and south of the equator we all remember,[1] meteorology, as a science, has made great strides; yet though the error of the historian of old in assigning cause for effect had long since been recognised and corrected, it is only within the last few years that any effort has been made to place the study of the science in question on a sure basis.

That this should be so at first appears strange to one reflecting on the intimate connection of the subject with the health, comforts, and daily pursuits of man; but the solution of the difficulty is easy, for no other science requires for the working-out of its problems so many data, and is so entirely dependent on the sister-sciences.

Consequently, it was not to be expected that, while hydrostatics, mechanics, and experimental physics, were as yet in a rudimentary state, any satisfactory progress could have been made in the generalization of the complicated laws of meteorology.

Now all is changed; and, owing especially to the introduction of telegraphy, the subject has received, and is receiving, the attention it deserves. A system of observatories has been organized, and societies have been founded at home and abroad, having for their object the reduction of observed facts to general laws, and the application of these laws in turn to inquiries conducive to the well-being of man.

Among such associations the British and Scottish Meteorological Societies hold a foremost place, and the journals published periodically by them give evidence from time to time of progress made and of work yet to be done.

In the numbers of the 'Journal of the Scottish Meteorological Society' which have been placed in our hands many valuable articles occur, of which we would briefly particularise the following:

1. On "Interruptions in the regular Rise and Fall of Tempera-

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[1] Lib. ii, chaps. 25, 26.

ture in the course of the Year," by Alex. Buchan, Meteorological Secretary.

This article, which is but one of a series, enters into a consideration of certain weather phenomena observed for a long term of years in the second weeks of November and December respectively. From carefully compiled records, it would appear that in the British Isles a remarkable fall of temperature takes place from the 6th to the 10th of November, while an equally remarkable period of warm weather occurs from the 4th to the 8th of December. From a medical point of view, the advantages likely to result from such investigations cannot be over-estimated, as it has been ascertained that the prevalence of many diseases, particularly those of a thoracic nature, and also the height of the death-rate, are much influenced by even slight variations from normal and seasonable temperatures. Hence, we regard as all-important any attempt made to anticipate such changes, and so to obviate their baneful consequences.

2. In a notice of the gales of September, 1867, in relation to storm-warnings, the necessity for obtaining daily weather telegrams from distant Norwegian and Russian stations is insisted on, as the information that such observations might furnish would prove invaluable in the forecasting of storms likely to fall on the western shores of Europe.

3. An extremely interesting paper on "Ascertaining the Intensity of Storms by the Calculation of Barometric Gradients" is to be found in the number of the journal for January, 1868. The author, Mr. T. Stevenson, F.R.S.E., shows that storms depend for velocity, not on mere lowness of barometric readings at a given place, but on sensible differences in pressure subsisting between places near each other, or, in other words, on the steepness of barometric gradients. He finds that in our "northern latitudes barometric mean gradients of one inch of mercury to 170 miles have occurred, and that such gradients produce atmospheric disturbances sufficient to endanger many of our structures."

A correct appreciation of these facts would tend to dissipate the erroneous views on the use of the barometer so common at the present day, and would go to prove the inestimable value of telegraphic weather information.

4. Although a paper on any meteorological subject should afford interest to the medical reader, yet it is to be expected that an exclusively medico-meteorological article should to a still further degree arrest the attention of the professional man. Accordingly we would make special allusion to a paper by Dr. Arthur Mitchell on the question, "Are Exceptions in the Distribution of Temperatures associated with Exceptions in the Distribution of Disease and Death?"

In seeking for an answer to this momentous inquiry the author avails himself of the reports of the Registrars-General of England and Scotland. From a careful examination of these he arrives at the following results:

(1) That any departure from mean temperature is succeeded by a corresponding departure from the mean rate of disease and death.

(2) That in summer an increase of temperature above the mean is followed by an increase of disease, the tendency at this season being to death by the abdominal viscera.

(3) That in winter a fall of temperature below the mean causes a rise in the rate of disease and mortality, the tendency to death then being by the organs of the chest.

(4) That while the effect of abnormal temperatures is almost instantaneously evident as regards disease, yet as regards death it may not appear for ten days or a fortnight—a fact due, perhaps, to delay in registration.

The writer illustrates his remarks by certain tables of temperature and mortality in London and Manchester, and suggests that curves should be adopted to show more plainly the relationship between death and temperature.

5. In the nineteenth number of the journal we find, by the author of the paper last alluded to, a communication on the "Cause of some of the Pernicious Effects of Polar Winds, with Remarks on the Method and Scope of Inquiries in Medical Meteorology."

The essay is suggestive, rather than exhaustive of the subject; yet in a short space the writer has succeeded in condensing much that is useful as well as interesting.

Two points insisted on appear to us especially to deserve the attention of our profession. The first is, that polar winds owe their peculiarly hurtful properties more to their *dryness* than to their actual *coldness*. The second is that this injurious action of dry air on the human frame is too often lost sight of in processes for warming and ventilating public buildings.

With regard to these axioms—if we may so call them—the following may not be out of place:

At Madras, the 28th of May, 1866, will long be remembered as a day of most intense heat, the thermometer on that day reaching the great height of 110·6° in the shade. At 4 p.m. the depression of the wet-bulb thermometer was no less than 35·8°, the dry-bulb being at the same time 109·8°. The per-centage of humidity was consequently but 16.

The note appended by the Meteorological Department at Madras to the observation for this "dies mirabilis" is to this effect:—"The late setting in of the breeze from the sea, and the *excessive dryness* of the air, as shown by the very low per-centage of humidity, rendered the day *scarcely endurable*."

6. An elaborate communication by Mr. Alexander Buchan, "On the Connection between Storms in the Western Atlantic, Europe, and Western Asia," deserves more than a passing notice.

But we would, on the present occasion, merely commend it to the attentive study of our readers. Illustrated, as it is, by several carefully drawn maps, it will fully repay perusal, and will be found to throw much light on a hitherto rather obscure subject.

Besides the articles above mentioned, the journal is replete with much interesting matter, and bears evidence to the vitality of the society of which it is the mouthpiece.

In conclusion, recognising, as we do, the importance of meteorology to all, and more particularly to those engaged in mercantile or agricultural pursuits and to medical men, we would fain see such bodies as the British and Scottish Meteorological Societies and the Meteorological Department of the Board of Trade working more in concert than they have hitherto done. For, valuable as each of these associations may be individually, to them as to most other things the old adage applies, "Union is strength."

ART. XVI.—*A Practical Treatise on Bright's Diseases of the Kidneys.* By T. GRAINGER STEWART, M.D., F.R.S.E., &c. Edinburgh, 1868. 8vo, pp. 188.

As the substance of this treatise originally appeared in the pages of this Review, in a series of original articles, it is not at all necessary to give a review of it, either to make known the views of the author or to commend it to the attentive perusal of our readers. Scarcely anything else is necessary than to notice the appearance of the volume, and the excellent style in which it is sent forth by the publishers. Still it is but fair to state that the preface refers to a more complete enunciation of the author's opinions than was attempted in the several memoirs published from time to time, and also to some rearrangement of them. Moreover, the present treatise is illustrated by seven plates, each containing several figures; one of the plates, besides, being coloured. "Three subjects of special pathological interest are considered in supplementary chapters;" those subjects are—simple fatty degeneration of the kidney; acute atrophy of the kidney, as sometimes associated with acute atrophy of the liver; and the nature of the waxy or amyloïd degeneration.

The varieties of the lesions known under the general term of Bright's disease, and recognised by Dr. Stewart, are three—the inflammatory, the waxy, and the contracting; a division that accords generally with that proposed by Virchow. It will be thus seen that the congestive form of Rosenstein, admitted also by Sir W. Jenner and others, finds no place in Dr. Stewart's category of Bright's disease. Again, the inflammatory and the waxy variety are subdivided into three forms or stages. Of the former, a state of inflammation, one of fatty transformation, and another of atrophy, are enumerated; the one stage following upon the preceding in the order mentioned. Hence he admits the occurrence of a small atrophied kidney as a more advanced stage of the large, mottled, fatty kidney, and as a quite distinct lesion from the cirrhotic contracting kidney.

Further, the large, smooth, fatty kidney of phthisis and tuberculosis, reckoned by some among the forms of Bright's disease, is by Dr. Grainger Stewart not so regarded. Its history is given in a supplementary chapter.

"Depurative disease," assigned as a better term by Dr. Dickinson to the state known as waxy kidney, is rejected by the author, whose experience is opposed to Dickinson's hypothesis of its production as peculiarly a consequence of suppuration and of the undue abstraction of alkali from the fibrin of the blood.

The treatment best adapted for each of the three principal varieties of Bright's disease is fairly set out. In the inflammatory form the author gives his countenance to Dr. Dickinson's plan of drenching the kidneys by copious draughts of water, a mode of practice which we cannot but think of doubtful expediency, at least in many cases.

The pathology of albuminuria has received an amount of clinical, chemical, and microscopical study of late years, such as few other medical topics have obtained; yet, withal, the reader of Dr. Grainger Stewart's book will remark many debatable points still existing, although he will derive, at the same time, much satisfaction at the concurrence of pathologists in the interpretation of the principal morbid phenomena, and at the skill and study that have so well unfolded the minute structural peculiarities of the several lesions accompanied by albuminuria.

Art. XVII.—*Clinical Lectures on the Principles and Practice of Medicine.* By J. Hughes Bennett, M.D. Fifth Edition. Pp. 1037. Edinburgh, 1868.

The publication of a fifth edition of this excellent book calls for a short notice which we had wished to insert in our last number, but were compelled unavoidably to postpone. Our good opinion of the general merits of the volume has been before expressed; so that it only remains now to draw attention briefly to the few additions and alterations contained in this latest edition.

The introduction has been re-arranged and certainly improved by the omission of some observations on the political and social state of the medical profession, and by the insertion of the substance of an address in medicine, delivered to the British Medical Association at Chester, in August, 1866. The wise words therein contained will still be fresh in the memory of most of us. We allude more particularly to the remarks on "the present state of the art of medicine," and on the necessity of "combined labour" for the better advancement of medicine. With regard to the former, it is suggested that the empirical method of treating disease has reached its limits; that the healing art is daily becoming more rational; that, at the same time, funds are urgently needed to organize well-selected and well-appointed committees of investigation to inquire into and report on the action of drugs; and that, lastly, all knowledge thus acquired, and all reports thus furnished, shall be tested fairly and in a scientific spirit by the practitioners at large. The co-operation of the profession would undoubtedly follow any such praiseworthy attempt as the author suggests. It must be evident to all that a rapidly advancing knowledge of pathology and an increasing accuracy of diagnosis are bringing with them an insatiable longing for a more intelligible, a more rationally applied, system of therapeutics. With regard to the combination of labour, we quote the author's own words:—"What we require is a legislation which, instead of maintaining a system of rival institutions and opposing schools, perpetuating disunion, and retarding the cause of scientific progress among us, will draw these discordant elements together, for the purpose of co-operation and mutual support." Before all things, therefore, it is necessary that there should be one recognised and adopted system of medical education. Second only to this in importance is the fusion of our numerous examining boards into one central and national board, by which all who will practise medicine in any or all of its branches must be tested. So long as there are twenty or thirty different medical and surgical diplomas, so long as degrees and diplomas can be purchased for a few pounds sterling, and flaunted in the faces of the victimized public, so long will the honest doctor and the sick man be exposed to the intrusions and annoyances of quackery.

The slight addition which has been made to the chapter on Tuberculosis, and which has reference to the inoculation of tubercle—a subject so full of interest to the medical world—had better have been omitted altogether than have been so imperfectly given. The author tells us that he inoculated with grey and yellow tubercle *five* rabbits, in the autumn of 1867, and that, on killing them three months afterwards, he found no trace of tubercle in any of the five, but soft cancer in the lungs of two. This appearance he believes to have been a "mere coincidence," and suggests, on the strength of this large experience, that it remains to be seen how far the tubercles said by other observers to be formed in consequence of tubercular inoculation, are not also the "result of coincidence." The student who reads this will hardly find himself "posted" in the subject of tubercular inoculation.

Professor Bennett stands out pre-eminent for honesty in his teaching of therapeutics. He has gone boldly out of the beaten track in this department of medicine; and, avoiding the routine enumeration of drugs, with a string of which, as "sometimes efficacious," it is customary to conclude the description of each disease in our medical works, he only advises the few remedies which he has himself proved, and not found wanting, in the wards.

The subject of Aphasia, now engaging so much attention, is briefly noticed. It is the author's belief that "all attempts to localise the faculty of speech have hitherto failed," and that the lesion in the anterior lobe of the left cerebral hemisphere, so often met with in cases of the kind, must be regarded simply as a coincidence.

The chapter on Uræmia is improved by some remarks and illustrations borrowed from Dr. Dickinson's now standard work. Averse as Professor Bennett is to bleeding generally in inflammation, he considers it to be "a valuable and necessary remedy" in uræmic coma, though he is not quite clear how its good effects are here produced.

The strenuous efforts so long made by the author to convince the medical world that uncomplicated pneumonia is not of itself a fatal disease, are being at length crowned with success. It is now quite the exception to see a case of pneumonia in any British hospital bled or treated with antimony in large doses. Clinical observation must show to every unbiassed physician the great advantages of the "restorative" over the antiphlogistic and all other plans of treatment.

Lastly, we hail with heartfelt satisfaction a considerable curtailment of the "bloodletting controversy" in this as well as in the previous edition. We sincerely hope that other polemical discussions of the kind will meet with the same fate in a future edition; that leucocythæmia and leukæmia have struck the last angry blow, and will henceforth keep the pathological peace, content to divide equally the honour of discovery to which they are both entitled.

PART THIRD.

Original Communications.

Art. I.

On the Various Forms of Loss of Speech in Cerebral Disease. By H. Charlton Bastian, M.A., M.D. Lond., F.R.S.; Professor of Pathological Anatomy in University College, London; Assistant-Physician to University College Hospital, and to the National Hospital for the Paralysed and Epileptic.

(*Concluded from page* 236.)

Physiological Explanation of the Phenomena presented in Aphasia and other modes of Defective Expression.

The theories that have been put forward in explanation of the 'aphasic' state are numerous, but owing to the various allied conditions not having been clearly distinguished from one another by some writers, and owing, as I think, to some others not having properly apprehended the kind and degree of relation existing between Language and Thinking, no small amount of confusion and contradiction is to be met with amongst these various explanations.

Omitting in this place a consideration of the views of the earlier writers on the various forms of loss of speech in cerebral disease, some account of which may be found in an interesting paper "On Aphasia," by Dr. Popham,[1] I will commence with the doctrine held by Professor Lordat.[2] This celebrated non-medical writer, who afterwards himself became aphasic, attributed the condition to a defect of co-ordination of the muscles which are used in the act of speaking —an explanation which, however, gives no account of the inability to write also exhibited by aphasics. It might with much more show of truth have been advanced as an explanation of that less common state which I have considered under the name Aphemia, though even then a lack of due co-ordination amongst the muscles of articulation should rather show itself in stuttering speech than in no speech at all. Dr. Marc Dax, in a paper read at the Medical Congress of Montpellier, in 1836, on "Lesions of the left half of the Brain coinciding with the loss of Memory of the Signs of Thought," indicates by the title of his essay that in his opinion the loss of speech is due to a loss of the memory of words[1]; but as I have before stated when this is the case, thought itself would be stopped to a similar extent—a condition of things characteristic of Amnesia but not of Aphasia. In 1833, Dr. Osborne recorded the remarkable case which I have cited at p. 229, and he attributed the loss of the faculty of speech to "forgetfulness of the art of using the vocal organs." It must be remembered that his case was one of Aphemia, and, therefore, the explanation given was much more suitable for this class of cases, than when it is applied, as it has been in recent years by M. Baillarger, as an explanation of 'aphasia' in its old, general sense. For this combination of states, the explanation is obviously too narrow, as it affords no solution whatever of the difficulty that at the same time there is a loss of the power of expressing thoughts by writing. But whether Dr. Osborne's view be the correct one concerning his own case, entirely turns upon the question, whether we have a *memory*[2] of the muscular movements of the vocal organs concerned in speech or not. M. Broca, in his now celebrated memoir "*Sur le Siège de la faculté du Langage Articulé*," and in a subsequent communication,[3] after recording his two cases, proposed to give the condition he had been illustrating the name 'Aphemie,' the essential characters of which were according of him, not a forgetfulness of words, but a loss of the memory to the modes of muscular co-ordination necessary for the articulation of words, with integrity of the organs of association, and an unimpaired intelligence. But in both these cases of M. Broca (though he does not give due prominence to the fact), there was also an inability to write, so that they strictly belong to the ordinary category of Aphasic cases, and this explanation is, therefore, inadequate. Trousseau[4] holds that "an aphasic individual suffers from verbal amnesia, so that he has lost the formulæ of thought." He says again—"In aphasia, therefore, there is not merely loss of speech, but there is also impairment of the understanding. The patient has lost simultaneously, in a greater or less degree, the *memory of words, the memory of the acts by means of which words are articulated, and intelligence.* But he has not lost all these faculties in an equal degree, for the understanding is less injured than the memory of the acts for producing sounds, and this latter faculty less impaired than that of remembering words." In fact,

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[1] 'Dublin Quart. Journ. of Med. Sci.,' Aug., 1867.

[2] 'Rev. pér. de la Soc. de Méd. de Paris,' Dec., 1820, p. 317.

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[1] Reprinted in 'Gaz. Hebdom.,' 1865.

[2] That is, a conscious memory, as opposed to an unconscious, organized memory (see 'Fortnightly Review,' Jan., 1869, p. 63).

[3] 'Bulletins de la Soc. Anatom.,' 1861, and in 1863.

[4] Translation of "Lectures" by Bazire, Part i, pp. 266—273.

Trousseau insisted again and again upon the assumed fact that, "in aphasia loss of memory plays the principal part." He saw that the explanation adduced by some that the defect consisted in a loss of the power of co-ordinating the speech movements, was inadequate since it failed to account, as I have just stated, for the coexisting inability to write, whilst he thought that the loss of the memory of words explained everything. "The patient does not speak because he does not remember the words which express ideas." But we have seen that thought is impossible without a linguistic symbol of some kind; in pure Amnesia there is, I believe, an arrest of thought in proportion to the degree of forgetfulness of words; but in Aphasia the thinking power is comparatively unimpaired, and, therefore, words must be recalled to the mind in some form, although the individual is incapable of expressing them either by speech or writing. Dr. Alexander Robertson[1] saw this objection to Trousseau's theory clearly enough, and he contended that the defect could not be an amnesic one, because the individual retained the power of thinking, and, we think, by means of words. He considers the defect to be one of transmission—"that there is a lesion of efferent fibres passing between the convolutions and the great co-ordinating centres, probably at some point of a line extending from the external frontal convolutions to the corpus striatum, so that voluntary motor impulses for the articulation of language cannot be transmitted. The *essential* morbid change is, therefore, *motor*, and not *mental*." He thinks also with Marcé that there are separate co-ordinating centres for the acts of speech and for the movements of the fingers, &c., in writing. And he adds:—"The two kinds of conductors may be side by side so that both will generally be implicated by the same lesion; but in some rare instances,[2] those escape which I believe to be concerned in the mandates for writing, so that the words present in the mind can thus be communicated." As will be seen further on, Dr. Robertson's view, so far as it goes, is a closer approximation to what I believe to be the correct one than any of the others have been, though he does not speak of the mode in which words do become nascent in the minds of aphasic individuals, or further develop his theory. Dr. Wm. Ogle,[3] as we have before seen, distinguished between amnesia and the ordinary condition of aphasia. The first form he considered under the name *amnemonic aphasia*, and the second under that of *atactic aphasia*.[4]

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[1] 'Journal of Mental Science,' Jan. 1867.

[2] Those which I have placed in the class *Aphemia*.

[3] 'St. George's Hosp. Rep.,' 1867, p. 94. Almost simultaneously, in his paper in the 'Dublin Quart. Journ. of Med. Science,' Aug., 1867, p. 4, Dr. Popham made a somewhat similar division, as far as names are concerned, though these names were applied quite differently. Thus, his (1) *Lethological or amnesic aphasia* appears to correspond to what I call Aphasia simply, whilst his (2) *Aneural or ataxic aphasia* corresponds to what I have called Aphemia.

[4] What precise signification Dr. Wm. Ogle gives to the term *aphasia* requires

With reference to the latter condition, he writes—"Not only must we remember words, but we must also remember *how to say them.*" Should this latter memory fail, he says, "we have a second form of aphasia which for distinction we may call *Atactic Aphasia,* the loss of speech being due to the want of co-ordinating power over the muscles of articulation." Dr. Wm. Ogle here seems to me to assign, in consecutive pages, two reasons which are totally distinct from one another, in explanation of the same morbid condition—to both of which, however, I have already alluded. Dr. Wilks[1] is a firm believer in the doctrines of Dr. Moxon,[2] that the left half only of the brain is educated for the carrying on of the acts of speech; he considers that speech is essentially a motor process, and that the defect by which it is lost is also a motor defect. He also believes that we cannot think "without the larynx and muscles of vocalisation passing in the imagination through a kind of inarticulate speech," a view somewhat similar to that of Professor Bain, and, as I think, open to the same objections.[3] His views in part seem to be not very different from those of Dr. Robertson which I have just alluded to, and they seem also closely to resemble those of Dr. Hughlings Jackson with regard to the

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some explanation, as it is not, at first sight, quite clear from his paper having the title "Aphasia and Agraphia." He treats of mere loss of speech under the head of Aphasia, and of loss of the power of writing as a parallel state under the name of Agraphia —which name he was the first to introduce. But he does not seem to be aware that most writers understand by the term Aphasia, *some loss both of the power of speaking and of writing.* This may be inferred, since he makes a definite statement (p. 100) to the effect that "Aphasia and agraphia are usually combined together." Most writers would have implied in some way, however, that inability to speak (an *aphemic* symptom) and inability to write (an *agraphic* symptom) together constitute the condition of *Aphasia.* And throughout his paper it seems to me that Dr. Wm. Ogle, in using the word 'agraphia,' employs it as a designation for that inability to write which constitutes one of the factors of the Aphasic state. I also use it in this sense, though I then generally employ the corresponding adjective; but, in addition, I have proposed that it should be especially used as a class name, under which it will be well to range those cases in which agraphic symptoms only, or in the main, are presented. It seems to me, therefore, that if the title of Dr. Wm. Ogle's paper had been "*Aphemia and Agraphia,*" and if it had been explained that under this title he did not propose to treat of 'Aphemia' or of 'Agraphia' as class conditions, but rather of aphemic and of agraphic symptoms, the component factors of what Trousseau and most others call *Aphasia*— there would have been less room for any misunderstanding concerning his own views, or chance of his misconceiving the views of others.

I make these remarks because, I am sorry to say, it seems to have occurred to Dr. Wm. Ogle that, in the note above referred to, I have treated him with unfairness, by not recognising that he had made previously the same class distinction which I there propose.

[1] 'Med. Times and Gaz.,' Jan. 18th, 1868.

[2] This 'Review,' April, 1866.

[3] It is fully admitted that the two phenomena do very frequently occur together; but then this coincidence seems to be nothing more than might have been expected, if we consider the extreme frequency with which processes of thought do find articulate utterance throughout every day of our lives. I doubt the *necessity* of this suppressed articulation in a process of silent thought, and regard it only as a superadded phenomenon which is very likely to occur.

connection between thought and language. Dr. Jackson[1] believes that in aphasia the power of voluntary expression is principally affected; that the defect is for the most part a motor impairment. He says also, "we think by help of words—*i. e.*, by acquired arbitrary signs—and these are motor processes." Seeing that the person can understand when we speak to him, he says that though speechless, the patient is not wordless, since verbal signs may be developed in him by our talking, though he cannot initiate verbal movements. He adds:—"The words lie I imagine on the right side. It may, however, be said, that the internal reproduction occurs on the left side of the brain, and that the patient *does initiate* verbal movements, and would actually reply if the damage near the corpus striatum did not prevent words getting out." It appears to me that some lack of clearness exists in this statement, which seems in part attributable to the view that Dr. Jackson takes concerning the way in which words become nascent when we employ them in thought. As I believe, they are not "motor processes" in such a case, but simply revived auditory impressions. In a paper on the "Physiology of Language," read before the British Association at Norwich this year, Dr. Jackson ranges the cases presenting defects in intellectual language under two principal classes: Class I, corresponds with what I have spoken of as *Aphasia;* and Class II comprehends the various *Amnesic* defects in which a lack of coordination is exhibited, cases in which there are "*plenty of words, but mistakes in words*"—a division practically corresponding with that of Dr. Sanders and of Dr. Wm. Ogle.

Dr. Maudsley[2] has lately, in an interesting and original communication, expressed more at length and very strongly certain views with regard to the physiology of Thinking, so that it will be necessary for me to give these a more thorough consideration. According to Dr. Maudsley there are two essential factors in intelligent speech, the idea and the motor act, and too great importance has been generally attached to the influence of names or words in thinking. He says: "the faculty or power of co-ordinate movement exists in the proper motor centre, and may be excited by the idea descending from the cerebral hemispheres above." This stimulus releases the movement which is latent (or potentially organised) in the centre; and the movement, in the case of speech, results in the production of the word or articulate sound answering to the stimulating idea. So far it will be seen that, according to Dr. Maudsley, thought is a process of ideation taking place in the cerebral hemispheres without the intervention of language or linguistic symbols, which latter he seems to regard as only the physical expression of the accomplished ideation, resulting from the subsequent stimulation of the motor centres. To this extent, therefore, Dr. Maudsley seems in opposition with the great majority of thinkers, who believe that thought itself, the actual process of ideation, cannot be carried on without the constant intervention of linguistic symbols of some kind. Perhaps seeing the difficulty of maintaining his former position, Dr. Maudsley almost immediately adds—"But there is another most important consideration, which has been almost entirely neglected, but which is of the greatest moment, namely, the influence which the motor centres, when functionally excited, have, not only on the nerves which go from them to the muscles, but on the higher cerebral centres with which they are connected by intercommunicating fibres. The faculties which have been organised in them by education may act, and probably always do act, upwards as well as downwards, and, indeed, may act upwards without acting downwards. So acting on the supreme cerebral centres, and thus declaring themselves in consciousness, they become what have been called *motor intuitions*, recollections of movement associations." Now the postulation of these "motor intuitions" does not seem to me to make Dr. Maudsley's doctrine a more tenable one, neither do I see how it is possible that these can "furnish us with the conception of the design of a movement before it is made; that is, in the case of speech, with the word before it is uttered." For the motor centre has first to be excited (by "the idea descending"), and when excited the centre acts simultaneously in both directions—downwards, in order to call into play the requisite combined muscular action for the production of the corresponding articulate word, and upwards to the cortical substance, so as to convey to the mind the impression of the word which is *at that very time being uttered.* Therefore, the word never intervenes in the process of thought at all, and any intimation of *its* existence reveals itself in consciousness only at the moment when it is being actually converted into a physical mode of expression. In fact, these "motor intuitions" seem to me in no way to differ from the impressions derived through the so called "muscular sense"—they represent what Professor Bain so fully describes as the "feelings of muscular movement"[1]—and I have elsewhere[2] endeavoured to show why these cannot be so instrumental in the process of thinking as they have been deemed to be by some. But we must endeavour to get at Dr. Maudsley's meaning a little more fully. Somewhat further on he says:—"It is, therefore, misleading to say we think by means of names. We think by the ideas which names call up in the mind, and by the motor intuitions through which the ideas get definite expression in

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[1] 'Med. Times and Gaz.,' Sept. 26th, 1868.

[2] *Concerning Aphasia.* 'Lancet,' Nov. 28th and Dec. 5th, 1868.

[1] It may be seen, however, from the next section of Dr. Maudsley's paper that he does not seem to regard these as merely different names for the same phenomena. But though speaking of them as if distinct, he does not attempt to show how they differ from one another.

[2] Art. on "The Physiology of Thinking" (loc. cit.).

appropriate movement." Now, taking the latter part of his sentence first, we have already seen that the motor intuitions cannot so much assist in the evolution of thought since they are only the accompaniments of what is or might be, the physical expression of actually accomplished thought. Hence Dr. Maudsley must rely mainly upon the first part of this sentence: "We think by the ideas which names call up in the mind," he says, and so far I thoroughly agree with him. But I am compelled to ask him, how is it (in a process of silent thought) that these names which are to call up the ideas reveal themselves to his consciousness? I say they become nascent in my consciousness for the most part as remembered sounds, but Dr. Maudsley does not even hint at any such mode of revival of words; indeed, from a sentence which follows almost immediately, it would seem that he does not consider words intervene in thought at all except as motor intuitions. He says:—"What we really have as the essential factor in the mind is not the word but the motor intuitions denoted by the word." Now as I have previously shown, if we take the mode of origin of the motor intuition to be such as Dr. Maudsley describes, namely, an upward response to an ideational stimulus—which may or may not be synchronous with a downward stimulus to muscles issuing in articulate speech; then, practically, it must be admitted that thinking does habitually take place without the aid of language. And the only other alternative, as it seems to me, offered by the argument is, that these "motor intuitions" actually take the initiative in a process of thought (since *they*, he thinks, are really the mental names which are to call up the ideas); but since these motor intuitions, by Dr. Maudsley's own showing, must have their origin in and proceed from the motor centres, this would be to transfer some of the most eminently mental functions, such as *recollection*, from the cortical substance of the hemispheres to the corpora striata and other lower motor centres, and also to make these bodies always take the initiative in processes of thought. Few, we think, would be inclined to admit this doctrine. I will only add that Dr. Maudsley thoroughly rejects the doctrine of Broca, and he thinks it most probable that the Aphasic condition is principally due to a lesion of some part of the motor centres, or of their communications with the supreme centres; whilst he also suggests that some of the phenomena of Amnesia may be explained on the supposition of a partial loss of the muscular sense.

Dr. Bateman,[1] in the last section of his paper, says he prefers using the word Aphasia, as a designation for "all cases where speech is abolished or suppressed, from whatsoever cause," fully recognising, however, that this necessitates "divisions and subdivisions in which all the shades and degrees of the affection may be included." He thinks a person may become aphasic in two principal ways, "either by losing the memory of the symbols of language, or by forgetting the mechanical movements necessary to give expression to such symbols." Lastly, Mr. Robert Dunn, in a paper read at the late meeting of the British Association,[1] ably insists upon the different situations in which lesions of the hemispheres may give rise to loss of speech, and he also, with Dr. Maudsley and others, rejects the notion that cerebral loss of speech does not result from lesions of the right half of the brain.

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[1] 'Journ. of Ment. Science,' Jan., 1869.

By this time the reader will pretty well have gathered in outline what my own views are concerning the phenomena exhibited by persons suffering from the various defects in thinking, and in the power of expression which I have classed under the heads of, 1. *Amnesia;* 2. *Aphasia;* 3. *Aphemia;* and 4. *Agraphia.* These may now, however, be stated more explicitly; and in order that such explanations may be more fully understood, it will be necessary to recall the reader's attention to views which are stated more in detail elsewhere[2] concerning the localisation of function in the cerebral hemispheres.

It seems almost certain that impressions from the organs of sense to the perceptive centres in the cerebral hemispheres travel along definite routes, although we may be more or less ignorant as to what these routes are, and also as to the extent and situation of the ground occupied in the cerebral hemispheres by the several 'perceptive centres.' We are ignorant, also, as to how far these centres exist separately, in areas distinct from one another, or how far and in what way they are interblended with one another. We may be sure, however, that such centres do exist somewhere, and are in connection with afferent fibres, bringing them into connection with ganglionic masses, existing nearer the base of the brain and the medulla oblongata; that upon these lower ganglionic masses sensory impressions travelling along their respective nerves impinge, and that each there undergoes that first modification of the series which is to convert the primary stimulus into a distinct Perception, after the final action of the cells of the cortical substance in the corresponding perceptive centre. And because strong associations grow up between impressions of one sense and those of another, so that *e.g.* sound impressions will recall to mind associated visual impressions, and the reverse, so we are entitled to infer that abundant communications exist, not only between these two centres, but between any single centre and all the others. As I have before stated, I believe that words are revived (during thought) in the auditory perceptive centres, and that from these centres the fibres issue along which the volitional stimulus must pass in order to translate the thought into articulate speech. It seems almost certain, also, that these efferent fibres in their downward course to the centres in the medulla oblongata, must pass through the corpora striata. In writing, I believe, the preliminary thought is carried on in the same way by the revival of words as auditory impressions, and that (by virtue of an association which was gradually matured whilst the child was learning to write) these revived sound impressions automatically revive the corresponding visual impressions of the letters composing the words, so that the motor stimulus which is to incite (automatically for the most part) the muscular movements necessary for writing, emanates principally from the visual perceptive centre and travels along a distinct set of fibres issuing from this, just as it is presumed those for speech do from the auditory pereeptive centre. There is every reason to believe that these fibres also pass through the corpus striatum.

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[1] Reprinted in 'Brit. Med. Journ.,' Jan. 30th, 1869.

[2] 'Journ. of Mental Science,' Jan., 1869.

Now, before going into further details, let us look at the explanation of the four different kinds of defect with the aid of these views. In *Amnesia* there is an inability to recall words—*i. e.* they cannot properly be revived in the auditory perceptive centres, and there is an almost proportional impairment of the thinking power. Now, it would appear that this condition must be due either to some abnormal state of the auditory perceptive centre itself, where words have to be revived, or else to some defect in those portions of the cortical grey substance which have to do with the exercise of that marvellous power of voluntary recall of past impressions to consciousness, which occurs in the process of *recollection.* In this condition we obviously have to do principally with defects of the cortical grey matter of the hemispheres, rather than with defects of afferent or efferent fibres connecting this with lower-nerve centres. But in *Aphasia,* as we have seen, the individual is able to think and understand what is said to him, though he cannot express himself either by speaking or reading. Now, we can well imagine that this would be precisely the condition of a person in whom those efferent fibres are damaged (and functionally inert) along which the motor stimuli are wont to pass that primarily incite those combined muscular contractions necessary, for speech on the one hand, and for writing on the other. There being no notable injury to the cortical or convolutional grey matter the individual can carry on processes of thought as before, and the afferent fibres not being damaged he can understand what is said to him. But he cannot translate his thought into articulate speech or into written language because the first part of the path along which the motor stimulus would have to pass, in order to incite the necessary combined muscular movements, is broken up or damaged. A lesion of both these sets of fibres in any part of their course between the cortical grey matter and the corpus striatum, or in this body itself, would, therefore, produce such a result. Just so, a lesion of either one of these sets singly would produce the corresponding simple condition of which Aphasia is compounded: thus, if the set of efferent fibres emerging from the auditory perceptive centre, having to do with the incitation to the muscular acts of speech, be torn across in any part of their course, we should have, I think, the anatomical explanation of *Aphemia*, in which, though the individual is able to think and write, he cannot speak; whilst, if the set of fibres emerging from the visual perceptive centre (along which, as I think, pass the motor stimuli that incite the muscular movements for writing) be alone damaged, in any part of their course between the convolutions and the corpus striatum, we have similarly the anatomical explanation of *Agraphia*, in which the individual can think and can speak, but cannot express himself in writing.

Assuming, for the present, that these explanations are not improbable, let us now see whether the various other characteristics presented by persons in the conditions above described accord equally well with the views above announced; and if they do, then must this accordance be taken, I think, as additional evidence in favour of the truth of what has been advanced.

It is not at all to be wondered at that *Aphasic* individuals, so long as their general intelligence is not notably diminished, should have the power of expressing themselves to others by *signs and gestures*, even though they have lost their command over the more definite modes of expression. For, although distinctions have been drawn between the loss of articulate speech, on the one hand, and the loss of the so-called "general faculty of language" on the other, it is very questionable whether this is a desirable way of stating the distinction. In the latter case, there is, necessarily, a loss of this supposed faculty, since a general weakening of the whole intellect to an extreme degree includes, of course, as one element, a loss of the power of Naming; which power, as Herbert Spencer has shown, depends upon the integrity of memory and the capability of performing the simplest acts of inference. And whenever the intellect is extremely weakened by a very severe cerebral lesion, the individual does lose this power of naming, and therefore it seems somewhat unfair to speak of a loss of the general faculty of language in such a case, when it should rather be stated more broadly and more simply that his memory is very much impaired, and that he is incapable of performing even the simplest acts of reasoning. In such cases the power of expression, even by signs and gestures, would be almost entirely absent, but we can well understand that in typical cases of Aphasia (where the memory is good and the intellect only very slightly impaired), although the individual may have lost the highly specialised power of expressing himself by articulate speech or writing, he may still show, so far as possible, the thoughts that are within by aid of emotional demonstrations and by signs. These are modes of expression common to man and the lower animals, and as such they have a far deeper and more organic root

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man, as not ... strivings which may be opposed to them—as may be seen when a
person is absolutely compelled to give way to laughter, even though
he make the strongest efforts to restrain it, and all prudential
reasons may be against the indulgence.[2] If this is the case, and if
we see that Aphasic individuals have the power, sometimes, by a
strong volitional effort, of uttering words which they cannot afterwards repeat, then it is not to be wondered at, I think, that these
same individuals should, under the influence of strong emotion, be
able to swear, or utter such other expletives as they may have been
accustomed to make use of, when in similar emotional conditions,
before they were rendered aphasic.

The emotional stimulus being a much stronger one than the volitional—a thing of higher tension—it forces its way along channels and against resistance which the volitional stimulus alone is utterly unable to overcome; for though the individual may swear or utter such an expletive as "Oh, dear!" at the incitation of emotion, he is quite unable to repeat the same phrase by any volitional effort that he may make, a few moments after, when the passion has subsided. I cannot, therefore, altogether agree with Dr. Hughlings Jackson, when he says, in explanation of this peculiarity of Aphasics, that their ability to swear depends upon the fact that there are two distinct kinds of language, emotional and intellectual, of which the latter only is impaired in the aphasic condition. Although it may be perfectly true, as Herbert Spencer says, that "cadence is the involuntary commentary of the emotions on the intellect," and that swearing, so far as its *effect* is concerned, does, as Dr. Jackson says, "belong to the same general category as loudness of tone and violence of gesticulation:" still I do not think he is correct in stating that "swearing is, strictly speaking, not a part of language." Seeing that oaths and expletives do consist of actual articulate words, they must be called up and evolved by precisely the same physiological methods as are

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[1] Using this in its broadest sense, so as to include the various appetites.

[2] Although I make this distinction here between action prompted by emotion and that prompted by volition, I am only using these words in their ordinary acceptation, and am fully alive to the fact that the two kinds of action merge insensibly into one another. Volition is a kind of sublimation from, and highly specialised form of, an ordinary emotional stimulus.

concerned in the enunciation of any more strictly intellectual proposition. The sole difference, in my opinion, is that, in the one case the words are evolved, in spite of opposing difficulties, by the superior force and energy of an emotional stimulus; whilst in the other they often fail to be produced under the influence of the more slender and feebler stimulus of volition.[1]

With regard to the case which Dr. Bateman has mentioned,[2] where the woman *mimicked* and repeated everything that was said, although unable to utter a single word spontaneously, it seems to me that it and similar cases cannot belong to the Aphasic class; they are rather instances of an extreme form of amnesic defect, in which the power of voluntary recall, or recollection, of words is wanting. When the impressions are, however, excited in the auditory perceptive centres, then these are capable of giving rise to the necessary motor stimulus which translates itself into articulate speech. The defect could not be, as in Aphasia, due to an impediment to the passage of the motor stimulus; and this supposition seems to derive confirmation from a consideration of the case of the artillery officer recorded by Dr. Hertz (p. 219), who, it is said, "was able to articulate distinctly any words which either occurred to him spontaneously, or when they were slowly and loudly repeated to him. He strenuously exerted himself to speak, but an unintelligible kind of murmur was all that could be heard. The effort he made was violent, and terminated with a deep sigh. On the other hand, he could read aloud with facility." This also seems to me a defect of just the same kind. He could utter a word which came to him spontaneously, and he could repeat words uttered before him; but he could not himself voluntarily revive the sounds of words in his auditory perceptive centre, so as to be able to recollect them and speak them. Similarly, with a book before his eyes, visual impressions were recognised; these revived their corresponding auditory impressions, and the volitional channel between this centre and the corpus striatum being uninjured, the motor stimulus passed over for the articulation of the revived words. The remarkable case recorded by Dr. Hun (p. 220) differs from this, but still seems to resemble it much more closely than it does ordinary cases of aphasia; the lesion was probably rather one of the grey matter than of motor fibres issuing from this.

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[1] Dr. Gairdner's explanations ('Trans. of Philosophical Society of Glasgow,' 1866) do not seem to me to be applicable to the cases which I class under the name *Aphasia*, though they are excellent if applied to the elucidation of the more or less involuntary utterances of *Amnesic* patients. He says, "The explanation that seems to me, amid the confessedly great difficulties of the subject, to come as near as any to a satisfactory one of these and other apparent eccentricities of utterance in the aphasic is, that the words which are most readily spoken are always those which are prompted, not by external observation and deliberate volition, but by some internal association of ideas, the words being not so much deliberately spoken as set free (so to speak) by the fluctuating waves of emotion or of memory, which may be conceived to act upon the old-established habits and latent capacities of the brain, very much as one wholly unskilled in music, or deprived for a time of his musical faculty, might let loose the tones of a barrel organ, without any direct personal cognisance of the music he is playing or the precise combinations of movement he is calling into action. In other words, the aphasic is able to pronounce those words, and those only, that are *called out of him*, as it were, by trains of association, of which he is only partially conscious at the time, acting upon forms and modes of expression which he may have learned to blurt out in the presence of similar associations long before his disease; while, on the other hand, he is utterly unable to select deliberately an appropriate term from among many inappropriate, and to apply it with free rational choice, and a corresponding determinate effort of volition, to the expression of a predetermined idea then and there accurately defined in the consciousness" (p. 19). All this applies perfectly to an *Amnesic* individual who has lost the power of voluntarily recalling, or recollecting words, but not to one *Aphasic*, who, when the word—even the simplest—is suggested to him, is utterly unable to pronounce it.

[2] 'Journal of Mental Science,' Jan., 1869, p. 501.

Most aphasic patients can *understand perfectly what is said* to them, and can follow and feel interested when they hear others read aloud. In these cases we may presume that the afferent fibres connecting the auditory centres of the medulla with the auditory perceptive centres of the cerebral hemispheres, and also these latter centres themselves are intact, so that the spoken sounds revive their accustomed impressions in the hemispheres, these being perceived as words symbolic of things or ideas, which, being duly appreciated by the individual as they are conjured up, suggest to him the thoughts which they are intended to convey. In certain of the severe cases of Aphasia, however, as in that by Dr. Bazire, recorded at p. 224, and in Dr. Gairdner's case,[1] it is distinctly stated that the patient either did not gather at all, or with difficulty and imperfectly, the import of words when he was spoken to, though he could be made to understand, with the utmost readiness, by means of signs and gestures. Must we not suppose that in such a condition either the communication of the afferent fibres with the auditory perceptive centres is cut off, or that this centre itself, in which the sounds of words are habitually discriminated and associated with the things to which they refer, is more or less injured? In either of these cases, though the sound is not appreciated as a *word* having its definite meaning, we must not expect that there would be deafness: the sound would be still heard as a *mere sound,* only it does not call up that superadded intellectual discrimination, by the ingrafting of which upon it it can alone be made to serve as a symbol of thought. Hence the individual does not adequately comprehend when spoken to, though he may be quite capable of receiving and appreciating fully the import of signs and gestures which make their impression upon his visual perceptive centres.

Some of these patients can *read to themselves,* and some cannot. In those who can we must suppose the afferent fibres going to the visual perceptive centres, these visual perceptive centres themselves, as well as the auditory perceptive centres and the fibres connecting the two, uninjured, and we then have the structural conditions necessary for enabling the individual to understand what he reads. As we have before seen, in the process of learning to read, the person has first learned to speak and to understand speech, by the gradual growth and strengthening of the primary memorial association between things and their names or sounds: then there is slowly superadded to this an additional association between certain visual impressions, made by the printed or written symbols of words, and this previous compound association, till at last the mutual association becomes so strong, that any one being given to consciousness —either the thing, the name, or the written symbol of it—each is capable of calling up automatically the other two. So that in reading, as I imagine, the visual symbols of words call up or revive automatically (by means of the connecting fibres between the two centres) the words as sound perceptions, and by the aid of the two impressions combined, but principally, I am disposed to think, by means of the second (although it obtrudes so little upon our consciousness) we gather the import of what we read, by the realisation of the third term of the association. Here, of course, the views I hold differ from those expressed by Prof. Bain, because I cannot agree with him as to the way in which words become nascent in a process of silent thought. He says—"It is not necessary to read aloud in order to transfer the work from the eye to the voice—a mere whispered or muttered articulation—a mere ideal rehearsal will become coherent. In fact, I believe we retain written language by the help of both methods, or by a combination of trains of symbols, as seen by the eye, with trains of symbols rehearsed by the voice." He thinks such a transference, therefore, "from the eye to the voice" necessary for the appreciation of printed or written language; but if it were so, reading ought to be impossible to Aphasics, since this is just the very transference which could not take place; and, in short, if we accept Prof. Bain's view with regard to the way in which words become nascent in consciousness—that "a suppressed articulation is, in fact, the material of our recollection"—then it seems to me a matter of the utmost difficulty for us to explain the aphasic condition and the individual's perfect ability to think whilst his powers of articulate speech have vanished.[1] It is this difficulty with which Dr. Hughlings Jackson has had to contend, since he has accepted Prof. Bain's views. In attempting to explain what we may meet with in another

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[1] 'Glasgow Med. Journ.,' May, 1866, p. 13, and 'Trans. of Philos. Soc. of Glasgow,' 1866.

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[1] Professor Bain himself says—"It is not very easy to produce any instance to show, on the other hand, that a series of actions can be repeated mentally and not bodily; for as the mental actions are performed in the same circles, it usually needs only a volition, often the removal of a restraint merely, to bring them to the full length of actuating the muscles." ('Senses and Intellect,' third edition, p. 346.)

Aphasic individual, viz., that he is unable to understand what he reads, Dr. Jackson says—"He cannot *voluntarily* repeat the words internally," though, "if we take the book and read to him he understands, so that an automatic reproduction of words is possible." Now, in both these cases, I think, the reproduction of words would be automatic, in the one case immediately, by an impression on the sense of hearing, and in the other secondarily, after a primary impression upon the sense of sight. And where the individual cannot read, I am inclined to think this must be owing either to some lesion of the afferent fibres to the visual perceptive centre, of the visual perceptive centre itself, or of the communications between the cells of this centre and those of the auditory perceptive centre. If lesions existed in either of the first two situations the visual impression could not receive its intellectual elaboration, and, consequently, it could not call up its associated sound (word) in the auditory centres, and hence no meaning would be conveyed by the hieroglyphic marks of the printed or written pages. They would be to the person mere meaningless strokes, just as we have assumed that if similar defects existed in the auditory perceptive centres, or in the afferent fibres with which they were connected, the individual could not appreciate the meaning of spoken words—these would be to him mere sounds. Unless on some such view as this, how can we account for the fact, so often met with—and of which we have a striking example in Trousseau's case (p. 226)—that an aphasic individual, otherwise exhibiting a marked degree of intelligence, is unable to point out individual letters, even when these are of the largest size.[1] If the communications between the visual and auditory perceptive centres were injured, there probably would be, at the least, a great impairment in the power of comprehending written language.

Then it is often found that, although the individual *cannot write* a word of his own accord, or, perhaps, at the most, only his own name, he *can copy* what is written for him, and he can often, as Dr. Jackson has shown, with a printed copy before him, transfer the words into the ordinary characters of writing. In this case, he may or may not understand the meaning of what he transfers, according as he is capable of reading to himself or not. Dr. Jackson thinks it is necessary that in this process the calling up of the "verbal sign" should intervene, and I think so too, if the individual comprehends what he copies, though we differ in opinion as to the nature of this "verbal sign." But I can quite conceive that an individual might have this power of transference to a limited extent, even though he did not understand the meaning of what he was copying, and in whom, therefore, the proper verbal sign was not called up. He would copy letter by letter, and simply effect the interchange mentally (by an operation performed in the visual perceptive centre) so as to substitute the written for the printed symbol. When persons are able to copy in this way words from printed into written characters, it seems to me a process comparable with the power which certain other aphasic individuals have of repeating sounds that have been previously uttered before them; and in both cases it seems to show that we have not to do with a pure case of Aphasia in which both of these modes of expression are no longer possible, but rather with one of a mixed nature, partly aphasic and partly amnesic. The individual cannot recall voluntarily the written characters for words; but when he sees them, or even printed characters, the impressions so revived in his visual perceptive centres do enable him to give rise to the motor stimuli necessary for writing them. His power of voluntarily recalling, or of recollecting, the written characters, is, therefore, at fault, rather than his power of executing the movements necessary for writing them. Seeing that such a condition may exist, it is also perfectly comprehensible that so long as the due communications exist between the auditory and visual perceptive centres, individuals who cannot write anything spontaneously may write when words are dictated—the auditory impressions, in these cases, automatically reviving the corresponding visual impressions which are capable of giving rise to the muscular movements necessary for translating them into written characters. Have we not here an explanation of the cases cited by Dr. Osborn and Marcé (pp. 221, 222)? Only in these cases we must suppose that in addition to their defective power of recalling impressions which had been made upon their visual perceptive centres, there was also some lesion of the *afferent fibres* pertaining to these centres, since, in both cases, impressions made directly upon their visual organs received no intellectual elaboration and were not comprehended. These individuals could not read, neither could they even comprehend their own writing—which they must just before have executed under the influence of impressions, which, having first received their intellectual elaboration in the auditory perceptive centres, must then have aroused the corresponding portions of the visual perceptive centre, and so allowed the proper motor stimuli to issue.

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[1] Since the above passage was written I have had under my care a patient who had been hemiplegic on the right side and aphasic, but who had in great part recovered. She could speak fairly well, she could understand perfectly what was said to her, and when passages of a book were read aloud to her, but could not read a single word herself: the *sight* of the words seemed to convey to her no meaning, and neither could she pronounce a single word from looking at it alone, though she did this at once after she had *heard* it pronounced. She could even scarcely point out single capital letters.

But these are quite exceptional conditions: the rule is that the individual who can copy more or less cannot write at all from dictation. In copying, as Dr. Jackson points out, the patient transfers letter by letter in a slow way, so that we have a mere imitative reproduction carried on through the instrumentality of the visual perceptive centre alone. But in writing from dictation, the process is a much more complicated one; the individual must be able to hear the words as well as realise the mental elaborations of the sounds, and in this process the impressions must be capable of calling up corresponding ones in the visual perceptive centres before the motor stimulus could issue for the execution of the writing. It is obvious, then, that the power of writing from dictation may be abolished by an imperfection in the communications between the auditory and visual perceptive centres, by a lesion of these latter centres, or of the efferent fibres proceeding from them to the corpus striatum. The latter condition is what I have presumed to exist in a typical case of Aphasia, where the individual knows what he wants to write just as he knows what he wants to speak, but finds himself, nevertheless, as unable to write a single word as he is to articulate the same. In these cases, too, the persons are quite unable to copy—they cannot write a single letter if this kind of lesion is extensive, and all attempts end in the marking down of mere meaningless strokes. But if their efferent fibres are intact, and there is instead a moderate lesion of the visual perceptive centre, or of the fibres connecting it with the auditory perceptive centre, then the individual might be able to copy, to transfer from printed to written characters, or even to write after a fashion with more or less mistakes of spelling and substitution of wrong words. There would then be amnesic and inco-ordinate defects in writing, rather than an absolute inability to write.[1]

These explanations accord exactly with the varieties met with in the *Agraphic* condition. Thus, in the case recorded by Dr. Wm. Ogle (p. 232), there was a notable amnesic defect of speech, though the patient could not write a single letter; and here we may presume that whilst there was an injury of the grey matter of the auditory perceptive centre in which words are revived, there was also an injury to the efferent fibres proceeding from the visual perceptive centre (in some part of their course between it and the corpus striatum) which brought about the inability to write even simple letters.[2] In the case of the woman, recorded by Dr. Jackson (p. 232), there was a smaller amount of the same amnesic defect of speech, and an inability to write any single word—even her own name—

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[1] I cannot understand Dr. Jackson's explanation of the inability of these persons to write from dictation. He says, "Although the dictated words do revive verbal signs, these automatically revive next, not images of *writing* letters, but concepts. If writing is to occur there must be *a further stage.* They would have to revive voluntarily the words, in order that he may transfer them to paper. * * * * When he copies verbal signs are revived too, but these develop merely the visual images of caligraphic marks."

[2] There may have been, also, some lesion of the visual perceptive centre itself, since this man was quite unable to spell by picking out separate letters, which he ought to have been able to do if the centre itself had been uninjured.

with any approach to accuracy, although the penmanship and shape of the actual letters was fairly good. Here I think there must have been some grave defect either in the visual perceptive centre itself, or else in the fibres connecting it with the auditory centre, whilst in Dr. Jackson's other patient (p. 233), who made mistakes in words and spelling, but still wrote infinitely better than the last patient, there must have been a slighter defect of the same kind. And in the case which I have recorded (p. 234) we can, I think, best explain the phenomena he presented by supposing lesions of these same commissural fibres to exist, either alone or combined with lesions of the visual perceptive centre itself. In this case, the man presented remarkable variations in his power and mode of writing, but then his general condition must be borne in mind, as it is such a common occurrence to observe in insane persons remarkable differences in their mental state at different times. It is worthy of note that in this case as well as in that of the government clerk recorded by Dr. Jackson, the mode of spelling was almost entirely in accordance with the mode of writing, and this is what we might have expected, I think, if we consider how it is that a child learns to spell, viz. in very great part by conning, and, by dint of frequent repetition, impressing upon its mind the visual appearance and collocation of letters as given in a spelling book. Still, before one had thought all this out, it was very puzzling to hear a man when asked to spell "cat" say deliberately "caudd," and then immediately pronounce the word as though he had spelt it "cat." Visual impressions obtained by reading and learning to spell do appear, also, to be called in as accessory aids when memory does not easily revive a wished-for word in the auditory perceptive centres. Thus, a most interesting case has been recorded by Dr. Graves,[1] in which, as he says, there "was a remarkably exaggerated degree of the common defect of memory observed in the diseases of old age, and in which the names of persons and things are frequently forgotten, although their initials are recollected."

The man was a farmer, fifty years of age, who had suffered from a paralytic attack, from which he had not recovered at the time of observation. The attack was succeeded by a painful hesitation of speech. His memory was good for all parts of speech except noun-substantives and proper names; the latter he could not at all retain. This defect was accompanied by the following singular peculiarity:—he perfectly recollected the initial letter of every substantive or proper name for which he had occasion in conversation, though he could not recall to his memory the word itself. Experience had taught him the utility of having written on manuscript a list of the things he was in the habit of calling for or speaking about, including the proper names of his children, servants, and acquaintances; all these he arranged alphabetically in a little pocket dictionary, which he used as follows:—if he wished to ask anything about a cow, before he commenced the sentence he turned to the letter C, and looked out for the word "cow," and kept his finger and eye fixed on the word until he had finished the sentence. He could pronounce the word "cow" in its proper place so long as he had his eyes fixed upon the written letters; but the moment he shut his book it passed out of his memory and could not be recalled, although he recollected its initial and could refer to it when necessary. He could not even recollect his own name unless he looked out for it, nor the name of any person of his acquaintance; but he never was at a loss for the initial of the word he wished to employ.

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[1] 'Dublin Quart. Journ. of Med. Science.'

And now, after this inquiry into the physiological explanations which, I conceive, may be offered of the various phenomena presented by the different classes of patients of which I have been speaking, I think I may be more clearly understood if I do not adhere so strictly to the more or less artificial classification of persons suffering from defective powers of expression into the four principal divisions—*Amnesia, Aphasia, Aphemia, and Agraphia.* These divisions were adopted for the sake of simplicity, and cases have been ranged under each of them, not because such classification was inevitable, but because, looking at them in their broad characteristics (and if they must be forced into one or other of these divisions) that division was selected which the case seemed most to illustrate. Now, however, I think I may venture to increase the number of these divisions without fear of confusing the reader, and I accordingly suggest the following eight groups of symptoms as more natural groups, under one or other of which most of the actual, or possible, cases can or may be ranged. Between the first group and the condition of health the transition is easy:—

Amnesic Defects.	1. An amnesic defect in speaking, but power of writing preserved: *e.g.* Case of Bouillaud, p. 230, and of Dr. Osborn, p. 229.
	2. An amnesic defect in writing, but power of speaking preserved: *e.g.* Case recorded by myself, p. 234.
	3. An amesic defect both in speaking and in writing: *e.g.* Case of Dr. Banks, p. 215, and of Dr. Jackson, p. 233.
Mixed Amnesic and Ataxic Defects.	4. An amnesic defect in speaking, with loss of power of writing: *e.g.* Case of Dr. Wm. Ogle, p. 232.
	5. An amnesic defect in writing, with loss of power of speaking: *e.g.* Case of Trousseau, p. 227.
Ataxic Defects.	6. Loss of power of speaking, but power of writing preserved: *e.g.* Case of Trousseau, p. 228.
	7. Loss of power of writing, but power of speaking preserved. I have found no pure case of this kind yet on record, and the nearest approximation I have met with was presented by the case alluded to in *note* 1, p. 484.
	8. Loss of power both of speaking and of writing: *e.g.* Case of Trousseau, p. 225, &c.

It will be seen that groups 6, 7, and 8 correspond, in the strict sense, with the conditions which I name *Aphemia, Agraphia,* and *Aphasia.* The cases illustrating the 1st group I included, in the body of the paper, under the heading *Aphemia,* those illustrating the 2nd, 3rd, and 4th groups (with the exception of the case of Dr. Banks), under the heading *Agraphia,* and those illustrating the 5th group would generally be ranged under the heading *Aphasia.* I look upon the classification first adopted as one suitable for clinical purposes; but that which I have given above is interesting rather as a scientific analysis.

ABILITY TO ARTICULATE IMPAIRED.

4 and 5.—Referring, now, to the table given at p. 211, it will be seen that the only other forms of "loss of speech" that I have to consider are those in which the *ability to articulate is impaired.* Many of these, indeed, can scarcely be considered as cases of loss of speech, they are rather cases in which different degrees of difficulty of articulation are met with. In these we have no necessary mental defects, and except in cases where hemiplegia of the right side exists, the individual is able to write, and express himself in writing perfectly. I shall allude to these defects only in a very few words, as they do not present many difficulties, and their nature is generally well appreciated. In all the cases the difficulty in articulating is associated, also, with more or less difficulty of *deglutition.* They are distinguished, also, by the fact that the utterance generally, if any be possible, is more or less thick and indistinct, thus differing altogether from what we have met with in the amnesic and ataxic defects just described, where, if the patient could utter words at all, they were articulated as clearly as possible. In the cases coming under these classes there may be difficulty of articulation alone, manifesting itself by different degrees of thickness of utterance, impaired power of moving the tongue, and difficulty of deglutition; or these impediments may be associated also with amnesic or ataxic defects of speech. When they co-exist these different conditions have to be discriminated. When the difficulty of articulation is slight, it often passes off completely in the course of a week or ten days, much in the same way as a slight facial paralysis may do, if the hemiplegia results from a lesion of some portion of one of the hemispheres or corpora striata. But in other cases the implication of the seventh and ninth nerves is much more marked from the commencement, and lasts a much longer time. This is often the case where the paralysing lesion occurs in the *pons Varolii.* And almost total loss of speech may be met with in connection with disease of the pons Varolii, occasionally without hemiplegic symptoms, though usually with paralysis of a marked kind. As an

instance of the former kind, I will quote a case which has been recorded by Dr. Wilks in his most valuable paper "On the Pathology of Nervous Diseases."[1]

Disease of pons: loss of speech.—A lady fell in a so-called fit during dinner. She was taken up speechless and put to bed. She lay with her mouth open and with the saliva running from it, and she was *unable to swallow or to speak.* There appeared to be no paralysis of her limbs, and from her gestures and expression there was every reason to believe that she was perfectly sensible. She was soon able to leave her bed, and recovered her usual health, but *she never lost the paralysis of the tongue and palate.* She wrote down all her wants on a slate. *She swallowed with difficulty,* and the saliva was continually flowing from her mouth; but she was able to walk three or four miles a day, and was accustomed to join in a game of cards. About two years after the first attack she had another apoplectic fit, in which she died. On post-mortem examination there was found to be a great amount of disease of the cerebral vessels; much blood, which had escaped from the pons, was effused at the base. Within the pons there was an old brownish cyst. The central ganglia were healthy.

Of course it is possible that this case might have been mistaken by an inexperienced person for a case of *Aphemia,* but the paralysis of the tongue and palate and the difficulty in swallowing really pointed most significantly to the seat of the lesion. I will now give the particulars of a case which has recently come under my own care as an out-patient at the National Hospital for the Paralysed and Epileptic, in which extreme difficulty of articulation was associated with well-marked and peculiar paralytic symptoms, such as induced me to diagnose a disease of the pons varolii. The history was obtained from the patient himself and from his son, both of whom were very intelligent.

J. W., æt. 54, married, and the father of six children, a plumber, but never had had lead colic or lead palsy; never had syphilis or rheumatic fever; heart healthy. About six months ago (end of last May) he began to feel pain in front of head and left side of body, coming on at intervals, for about two or three weeks. During some time, also, he was unable to taste his food properly. One day, whilst at work, he suddenly found he could not use left hand at all, so that he gave up working, and went home (which was close at hand). In about ten minutes he is said to have become completely paralysed on both sides of the body, whilst his face was drawn to the right side. This was attended by no convulsions, and even no loss of consciousness, but he found himself unable to speak or utter a single articulate sound. This condition of general paralysis having lasted one month, he then gradually began to gain power over the right side. During the first three weeks he seemed to be quite deaf, but during this time he could understand perfectly what was written for him on a slate. After this time he gradually regained his power of hearing, and became also able to utter single words when asking for food. Even at the end of the first month (whilst still unable to speak) he could write answers well to questions concerning his business, only rarely having any difficulty in recollecting words. He gradually improved in his power of articulating during the second month, though his voice was still muffled: and ever since he has spoken indistinctly and in a low voice, though with no difficulty in finding words so long as he is not excited.

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[1] 'Guy's Hospital Reports,' 1867.

The sight of the left eye was much impaired at first, and still continues bad. The sense of taste was also lost at first, and he has not yet regained it on the left side. Sense of smell on the left side is also much impaired. *During the first month he could not move his tongue at all out of his mouth, and had very great difficulty in swallowing. Even still is obliged to have his food cut up into very small pieces.* At, and immediately after, the time of attack it was found that he was extremely sensitive, and unable to bear the slightest touch without pain on the left side of the face, the left arm, and the left half of chest and abdomen; but over the left leg the sensibility seemed altogether lost. The sensibility of the right side of the body was natural. This state of things has continued, with very little change, ever since; but during this last two months he has been regaining sensibility on the inner side of the thigh and leg. He now complains of constant, though exacerbating, pain over the whole of the left side of head. The whole left side of the body has generally felt colder than the right.

Having begun to regain power over the right side after the first month, by the end of the second this was completely restored. Soon after this he began to gain a little power in the left leg, which has since slowly improved; now can walk about one quarter of a mile with aid of stick, dragging left foot along the ground. Within the last week or so has been gaining some slight power over the muscles of the left shoulder, but still cannot move his fingers in the least. During whole time of illness has passed evacuations naturally. He very frequently cries and sobs violently; rarely laughs; this from first, though increased of late. Appetite good, but sleeps badly, being very restless.

This case is an important one in many ways. It affords, in the first place, a good instance of the kind of loss of speech which is met with in disease of the pons Varolii, showing how it is associated with extreme difficulty in deglutition, and gradually gives place to a condition in which mere difficulty of articulation becomes a most unmistakeable feature. The fact of the paralysis for a time being bilateral, of the grave and persistent modifications of sensibility on the left side of the body, as well as the existence of deafness and the impairment of the sense of taste, taken together with the difficulty in articulation and deglutition, present a set of symptoms pointing almost indubitably to a lesion of the pons Varolii. The paralysis of the face in these cases is often on the side, opposite to the paralysis of the body, but it is by no means invariably so, since the lesion may be principally in the lower half of the pons and affect the root of the facial after its decussation, and just before its emergence from that part. Convulsions, also, and loss of consciousness, which are such frequent accompaniments of damage to the pons Varolii, do not occur at all in a considerable number of cases—the presence or absence of these depends much upon the mode and extent of damage inflicted upon this most important part of the brain.

6. The cases included under this class of *glosso-laryngeal* or *glosso-labio-pharyngeal* paralysis present characters so distinct that, when the observer is aware of their existence, he is not likely to confound them with others. The slowly commencing difficulty of articulation and difficulty of deglutition set in, perhaps, at first without

paralysis of limbs or wasting of muscles, though after a time the latter, at least, rarely fails to show itself, especially in one or both of the upper extremities. But whilst the difficulty of articulation and of deglutition may advance to such an extreme degree that the patient can scarcely utter even a sound or swallow a mouthful of fluid without danger of suffocation, the mental powers often continue quite unimpaired, and the person may be able to express his or her thoughts perfectly in writing; for in this affection we have a paralysis only of certain motor nerves taking origin from the medulla oblongata, accompanied by more or less of the muscular wasting to which I have previously alluded. The paralytic symptoms are due to an impairment of function, to various degrees, and in various combinations in different cases, of the hypoglossal, the spinal accessory, part of the vagus, and part of the facial nerves.

Various local diseases of the larynx itself produce *Aphonia*, and thereby induce a voiceless, though not necessarily a speechless condition. Such states may be occasioned by tumours of various kinds, stretching or pressing upon the recurrent laryngeal or motor nerves of the larynx, or owing to the growth of tumours from the vocal cords, and other chronic diseases of the larynx. Patients suffering in this way can generally make themselves understood by a faint and more or less indistinct whispered articulation. A consideration of these affections does not fall within the scope of the present paper, and are only now alluded to for the sake of reminding the reader that such mere voiceless conditions must on no account be confounded with any of the various forms of 'loss of speech' due to cerebral disease.

ART. II.

Notes of a peculiar form of "Granular Degeneration" observed in a Medulla Oblongata. By W. B. KESTEVEN, F.R.C.S.

THE following marked appearances were observed in preparing transparent sections of a medulla oblongata that was given to me two or three years ago, by Mr. Thomas Bryant, for purposes of histological study. Unfortunately, the clinical history has not been preserved; as, however, so far as I know, the diseased condition here shown has not before been described, I have thought it worth being put on record. Out of a large number of specimens of medulla oblongata and spinalis, both healthy and diseased, that I have examined I have met with nothing similar to what is observable in this instance.

The appearances in question consist in a number of minute cavities which, when looked at through transmitted light, by an ordinary pocket lens, or even by the naked eye, look like perforations (fig. 1). Viewed under low powers of the microscope they have the aspect of tubercle-like bodies (I use the word tubercle in its loosest sense), not unlike grey miliary tubercle of the lungs, as to their physical characters. Examined, however, more closely, by higher powers (eighth- or quarter-inch object-glasses), these are resolved into minute cavities, varying in size from $\frac{1}{180}$ to $\frac{1}{316}$ of an inch, either containing a fine granular substance, or entirely empty. In the latter case they seem to be scooped out of the tissues, and to be bounded by irregular edges, consisting of the fibres, &c., of the columns or grey matter. Multipolar cells and their processes may be seen crossing these cavities and their contents when the plane of section is on the lower surface (fig. 2); when the section has laid open the cavity at its upper plane it is either found to be filled with the granular matter, or to show the end of divided processes and fibres (fig. 3) in the same way as may be seen to have been effected by the

FIG. 1

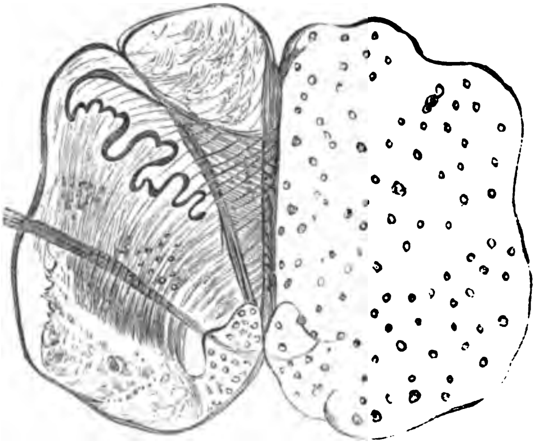


FIG. 2.

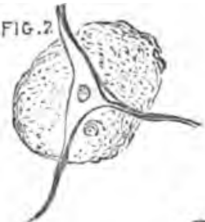
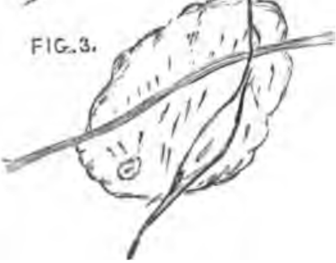


FIG. 3.



knife in ordinary sections. By varying the focus these several aspects are observable. In a few instances, the planes of section have passed both above and below the centre of the cavity, leaving a real hole in the textures. The contents of the cavities, when the contents are found, present the microscopic character of the "granular degeneration," so frequently observable in chronic disease of the spinal cord, and which was first pointed out and described by Mr. Lockhart Clarke. In a few of these cavities I have observed what I have supposed to be cellulose or amyloïd bodies.

These cavities are irregular, scattered, without evidence of order, throughout the medulla oblongata referred to. They cannot be said to predominate specially in any one of the elements of the organ—but if they prevail at all in any part, it may perhaps be said that they are rather more numerous posteriorly than anteriorly. In one section about the level of the calamus scriptorius, I counted several hundred of these cavities.

The surrounding textures appear to be perfectly healthy, with entire absence of any inflammatory action; neither is there sign of disease of the blood-vessels in the surrounding tissues. The morbid change is restricted to these detached points, and it is wholly a matter of conjecture whether it commenced in the capillaries, or in nerve-tubes or cells. Both these structures have undergone the processes of degeneration and disintegration, the cavities left thereby being occupied by a fine granular substance, which under high powers of the microscope appears as minute dots or points in a yellowish translucent plasma or pabulum. This I have intended to represent in fig. 2.

ART. III.

Miscellaneous Contributions to the Study of Pathology. By JOHN W. OGLE, M.D., F.R.C.P., Physician and Lecturer on Pathology at St. George's Hospital.

CHAPTER II.

Containing the History of a Series of Fatal Cases of Idiopathic and Traumatic Tetanus, and of Cases which may be called Tetanoïd.

(*Continued from* vol. xlii, p. 511.)

CASE 28.—*Traumatic tetanus.*

Clara G—, a girl, was admitted into St. George's Hospital January 11th, 1864. On the 26th of December she trod on a piece of bone, and thus received a wound in the ball of the toe of the right foot.

Some inflammation set in, and on the 7th of January the mother noticed a peculiar expression about the face. In forty-eight hours she complained of pain about the lower jaw, and at four a.m. on the morning of the 10th she had lost the power of opening the mouth. In the evening of the same day there was some opisthotonos, and when the patient was admitted the jaw was partially but fixedly closed. She had the sardonic grin of tetanus, and there was slight opisthotonos. The pulse was small and quick. No hardness or painfulness existed about the wound. The posterior tibial nerve was divided by Mr. Lee.[1] On the following day the sardonic grin was less marked; could she open her mouth about half an inch, and protrude her tongue. There was no dysphagia as to fluids; but no solids were tried. Any sudden movement of the bedclothes would throw the patient into a tetanic state. The pulse was 160, and regular; the respiration 38 per minute.

Two doses of calomel (gr. iii each) were given, but as no aperient action followed, enemata were administered. The patient varied much until the 16th of the month, when a very severe attack of general tetanus with opisthotonos, lasting four minutes, came on, in which she became blue, and quite insensible to all external objects. Afterwards she fell into a sleep, during which she had many slight spasmodic attacks, which did not awake her. On the 17th the attacks of spasm were very slight, and occurred, on an average, at the rate of seven per hour. On the following day there was some pleurosthotonos. She became weaker; still no spasm followed the act of swallowing liquids. At mid-day on the 20th she had a very severe attack of tetanus, followed by several others equally severe, and she died at 2.20 o'clock on this day.

Post-mortem examination.—*Cranium.*—The brain substance was natural. The pia mater was blood-stained around the vessels; but the membranes were natural. *Spinal column.*—The spinal arachnoïd cavity contained about half an ounce of blood-stained fluid, and the surface of the spinal cord was greatly injected, especially about the lumbar swelling, the grey matter on transverse section being at this part congested. Dr. Dickinson, who made the examination, observes that general sections of the cord were examined by

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[1] We have lately had in St. George's Hospital a fatal case of tetanus following a deep wound in the palm of the hand, in which Mr. Lee, at my suggestion, divided the median nerve, but this was very many days after the injury, and the tetanic condition had existed several days. The patient died a few hours after the operation. I may here say that Mr. Wilkinson, of the veterinary department of the army, has recently informed me that in the case of horses they scarcely ever find division of the nerve produce any good in traumatic tetanus. Cases of tetanus owing to cold, &c., are often saved, but not so traumatic cases. Traumatic tetanus in the horse is, however, recovered from. Several such cases are recorded, for example, by the late Mr. John Field, veterinary surgeon, in a little volume containing interesting cases of all kinds, entitled, 'Posthumous Extracts from Veterinary Records,' 1843.

Mr. L. Clarke, who found that the fissures of the cord, particularly the anterior, were occupied by exudation. *Thorax.*—The lower lobes of both lungs were hepatized, and the bronchial tubes loaded with mucus. The left ventricle of the heart was contracted; the right one being *un*-contracted. The other organs of the body were natural.

Remarks.—Notice the coming on of the peculiar expression of the countenance on the 13th day after the injury. This appearance was probably the result of muscular rigidity in some part of the face, which became more completely expressed in the pain of the jaw and the inability to open the mouth, which quickly followed. The freedom from spasm whilst swallowing liquids is a fact worthy of attention.

Notice also the facts of the *exudation* found in the substance of the spinal cord after death. With regard to this last point (not anticipating the observations which I propose to make hereafter), I would draw attention to the fact that within the last few years, thanks to the varied facilities which have been introduced of late in examining the histological characters of morbid structures, many observers have met with formations of new material within the substance of the spinal cord in tetanus as well as in other diseases, such as chorea, paralysis agitans, general paralysis of the insane, the so-called locomotor ataxy, wasting palsy, &c., in which the existence of such formations was not anticipated. Thus, Rokitansky discovered growth of connective tissue in the spinal cord in tetanus, and Wedl records a case of fatal tetanus, on the 12th day after injury, in which the ulnar nerve-branches in the palm of the hand were found imbedded in grey exudation, and high up the nerve was found to contain much blood, the grey matter of the spinal cord in the part corresponding to the brachial plexus being of a deeper red colour than in other portions. He suggests that in this case possibly the tissue of the cord was pervaded by exudation. Wedl also suggests that more attention should be paid to the state of the walls of the blood-vessels of the spinal cord in tetanus than is ordinarily given. Subsequently, in 1860, Dr. Demme met with increased development of the connective tissue of the spinal cord in tetanus, and in other spinal neuroses, such as wasting palsy, locomotor ataxy, &c., also of the brain-substance in certain cerebral diseases, such as epilepsy and idiocy.

Wagner also encountered similar changes in the spinal cord in tetanus. Fleckner in 1861, and Eisenmann and Oppolzer in 1862, met with similar growths in the structure of the axial nervous masses in like cases. Remak and Froriep found that in traumatic tetanus exudations occur at the injured part of the nerve, and also in a direction upwards along the larger nerves. Wunderlich found in a fatal case of tetanus of only four days' standing that the new formation of connective tissue had been produced in the spinal cord. As before said, Mr. Lockhart Clarke found various minute structural changes in the spinal cord in tetanus, in muscular atrophy, &c., and Dr. Dickinson has lately described a case ('Med. Chir. Tr.,' vol. 51) of tetanus in which such changes were found.

In hydrophobia again Cunningham found softening of the pons Varolii; and in paralysis agitans Stoffella found induration of the pons Varolii, medulla oblongata, and parts of the spinal cord, resulting from the formation of embryonic connective tissue. Bourdon has found the like in cases of locomotor ataxy. In chorea again microscopical morbid alterations, as well as those which Tenner and Kussmaul denominate "coarse" changes, have been met with in the spinal cord; to some of these I alluded in a previous communication on that disease to this Review (see the number for January, 1868, p. 228). Most probably all such minute changes of structure (whether they be of the nature of what are termed degenerations, or such as are manifestly new growths or so-called deposits or exudations) are due to modifications or perversion of nutrition, to which later on I shall allude. Of course such changes are not always to be met with. Thus, in the next case which I shall relate (No. 29) Mr. Pick was unable to find any such; and Billroth reports that in five fatal cases of tetanus he neither found neuritis nor any alteration in the spinal cord. It may be that our remedies are then only serviceable when such ulterior histological changes have not been established, or have not far advanced.

CASE 29.—*Tetanus following injury to the hand.*

Michael B—, æt. 28, was admitted into St. George's Hospital Oct. 22nd, 1866, with a smashed hand caused by machinery. The index finger and part of the metacarpal bone were removed, and for the first few days he did well. On the evening of the 30th (eight days after admission) he complained of feeling the jaw stiff, and shortly afterwards he had a slight rigor. On the day afterwards the jaws were more rigid, and in the afternoon firmly locked; he had also opisthotonos and pain in the epigastrium striking to the back. The face was congested, deglutition was difficult, and attended with spasm. One forty-fifth of a grain of atropine was injected subcutaneously at 4 p.m., and again at midnight. After the first injection the pulse was 144, and the spasms relieved. He complained of thirst, and there was slight "risus sardonicus" and the wounds looked dusky. He was ordered to have six ounces of brandy. The temperature in the axilla was 100.4°. A twentieth of a grain of atropine was again injected, and beef-tea and brandy given in enemata, but quickly returned. The pupils were dilated, and delirium supervened, but the opisthotonos and trismus became less marked. The atropine was injected in doses of one twentieth or one fifteenth of a grain daily for seven or eight days. On the sixth it was discontinued; epistaxis came on, and the jaws fell open between the paroxysms. The respiration was 30 per minute. Ice was applied to the spine and back of the head, but he got weaker, and *offensive perspirations* occurred. Sordes came on, the temperature being 100° and 101,° and the pulse became very feeble. The spasms became much more violent, and so much so that it was thought necessary to perform laryngo-tracheotomy, after which he was much easier, but soon afterwards he sank asphyxiated in a severe spasm.

Post-mortem examination.—The spinal cord alone was examined by our curator, Mr. Pick, who informs me that he found nothing wrong in its structure. The note of the examination which he kindly sent me was as follows:

"Upon laying open the spinal dura mater, which itself appeared to be natural, an excess of slightly turbid fluid was found in the arachnoid cavity. The pia mater was remarkably injected, and presented throughout its entire length a network of enlarged vessel. The cord was of the same consistence throughout, and no part could be found which was softer than the rest. On section it was found to be remarkably congested throughout, so that the white matter was of a decided pink hue. Microscopic sections were made from various parts by the method introduced by Mr. Lockhart Clarke; but beyond the fact that the blood-vessels were much enlarged and distended with corpuscles, no change in structure could be discovered."

Remarks.—Notice the occurrence of stiffness of the jaws on the eighth day as the initiatory symptom: also the offensive perspirations.

Case 30.—*Tetanus following an epileptiform attack.*

William E—, æt. 37, was admitted into St. George's Hospital August 3rd, 1866. He was a gardener and intemperate, but well nourished, and complained of cold and sore throat five days before admission. On the day before admission he was found in an epileptiform fit, and when brought into the hospital he was helped upstairs, and whilst in the ward was seized with tetanic spasms and opisthotonos. He recovered from this attack, and was put to bed. Ice-bags were applied to the spine, but shortly afterwards he was seized with a more violent attack of tetanic spasm, accompanied by trismus and opisthotonos. He was conscious throughout the attacks, and signed to the attendants to put the spoon in his mouth. He never rallied, but died in this attack, on the day of admission.

Post-mortem examination.—*Cranium.* The brain was wet generally, and slightly congested, and the puncta vasculosa numerous. Nothing more of importance was observable about it. The spinal cord itself appeared to be natural, both to the naked eye, and when examined *microscopically*, but on the pia mater of the cord were found scattered here and there thin, flattened, semi-transparent plates of calcareous matter, very hard and of a white colour.

Thorax.—The lungs were congested, and in parts contained patches of extravasated blood. The liver was fatty and the kidneys congested. The other organs of the body presented nothing of consequence.

Remarks.—Notice the freedom of the spinal cord from congestion. [225.]

CASE 31.—*Tetanus of doubtful origin. Softening of the Spinal Chord.*

John H—, æt. 12, having had a fall and grazed his arm two weeks previously, was admitted into St. George's Hospital Feb. 25th of the year 1867, in a state of acute tetanus, with severe opisthotonos, very often being supported in bed merely by the head and the heels. The sardonic smile was well marked, and the whole face much congested, and during the severer spasms very livid; at these times the jaws were quite locked, but open during the intervals. He complained of severe pain at the epigastrium and about the larynx. Slight priapism also existed. The skin was perspiring, the tongue white and furred, the pupils dilated and fixed. The pulse was small and 160 per minute, the respiration 44. The temperature in the axilla was 100° F. Brandy and beef-tea and a turpentine enema were given; and shortly after he came into the hospital one eighth of a grain of bimeconate of morphia and one eightieth of a grain of sulphate of atropine were injected subcutaneously. In fifteen minutes the patient fell asleep, but acute spasms again recurred, and he died on the day after admission.

Post-mortem examination.—The body had a natural appearance, and no indications of injury or abrasions existed in any part; the rigor mortis was well marked.

Cranium.—The bones were natural, the entire brain was exceedingly congested, and the "puncta vasculosa" much increased. The white cerebral substance was of a pinkish-blue colour, and the sinuses at the base were turgid. The brain was of firm consistence.

Spinal Column.—The vertebræ were natural. The spinal cord was congested, especially in the brachial and lumbar regions, where numbers of varicose vessels were seen. Opposite the eighth dorsal vertebra a portion of the chord was *decidedly softened,* though not to any great extent.

Remarks.—Notice the increased temperature of the body; and also the softening of the spinal cord after death. [49.]

The following fatal case I watched, now many years ago, along with the late Dr. Marshall Hall. I took the accompanying notes at the time. Unfortunately no post-mortem examination could be obtained.

Case 32.—*Tetanus without assignable cause.*

A young man, æt. about 30, a paper seller, had been in good general health, and without any apparent cause, was affected, May 18th, 1852, by "slight stiffness" in opening his mouth, which so far increased on the 19th as to cause him to eat with difficulty. Subsequently the lips began to be firm and stiff, and the right eyelid to swell. After that the muscles about the neck and upper parts of the chest became stiff and rigid, but the extremities were never affected. These symptoms continued until the 23rd, when difficulty in swallowing came on, and when I saw him, on the morning of the 25th, I took the following notes:—He was lying in bed, with a look of very excessive anxiety and dismay. His face was red and blotchy, and his eyes bright and active. The veins of the forehead and upper part of the face were somewhat distended, and the platysma—myoïdes, and sterno-cleido mastoïd muscles on both sides were equally and very firmly rigid. The jaw was not fixed, but could be slightly opened, so that the tongue could be seen to be clean. Attempts to swallow were ineffectual, although the larynx moved. The respiration was natural, and the skin comfortably warm and covered by perspiration. The pulse was firm and regular, but rather wiry. The power of motion and sensation in all the limbs were entire, but the abdominal muscles were very rigid, and the superficial epigastric veins were very distended. Moreover, on moving the bedclothes so as to expose the belly, the abdominal muscles of the trunk were spasmodically arched forwards. This state of spasm very shortly subsided, but recurred two or three times even without any fresh motion or adjustment of the clothes. The mental faculties were unaffected, and the patient answered questions rationally but in a whisper. No distinct pain was complained of, except that which accompanied the spasmodic attacks. The bowels were freely open, and three large doses of the tincture of hyoscyamus, the last one being in beef-tea injection, had been given.

May 26.—Night passed very restlessly, and no sleep obtained. Beef-tea injections, with tincture of hyoscyamus, had been given every three hours. The muscles of the neck and abdomen are the same, as also the superficial veins of the forehead and abdomen; but there is noticed a slight difficulty in raising the eyelids as high as natural, especially on the right side. At times he has violent spasm of the left side of the body and of the left leg; but the attacks are not lengthy, and the left leg is not permanently rigid. No twitchings of the fingers or limbs, or any other local spasm noticed by friends. Any attempt to get anything into the mouth produces a compressed and "leather-like" condition of the lips, endangering the efforts of inspiration; and on any attempt to swallow, the muscles of the larynx are seen to move not very readily. The bowels are open freely, and urine freely passed. The pulse is natural, but not quite so strong as it was yesterday, and the skin warm. The mental faculties are natural, and the patient writes down on a slate what he desires, though he can speak slightly.

On the 27th the muscles of the neck and abdomen were less rigid, and the enlarged veins diminished. The patient himself said that the left leg was not nearly so much contracted. The mouth was opened better, the speech more distinct and audible, and the swallowing easier. The right eyelid, however, was rather more closed, and the pulse was quicker and weaker. The hyoscyamus in beef-tea injections had been repeated. The same opisthotonic convulsions occurred on moving the bedclothes as before, but no noises, as of the doors, or clapping of the hands, &c., produced them. Although asked, the patient had no desire for ice. After a time difficulty in breathing came on, and frequent, but very short, attacks of opisthotonos; and this went on until about three a.m. of the 28th, when the patient died. Unfortunately, no post-mortem examination was permitted.

Remarks.—The chief points of interest connected with this case which I will briefly notice are—1. The apparent freedom from any traumatic or other cause of irritation. 2. The remittance in the severity of the symptoms which occurred. 3. The power of speech which remained until quite late in the history of the attack. 4. The limitation to a great extent of the opisthotonic convulsion to one side of the body at one time. 5. The continuance of easy respiration until within a few hours of death. 6. The order in which the most important symptoms connected with the state of the muscles came on, as we had first of all the stiffness of those which affect the movements of the jaw; secondly, those of the neck; then those of deglutition, and then those of the abdomen; whilst those of the limbs were quite unaffected. 7. The peculiarity in the swelling, and want of power in opening the eyelids; but these were most probably owing to effusion of serum into their areolar tissue, consequent on the distension of veins by pressure from the muscles. 8. The method of death, which appeared to be by spasm of some or all of the muscles of respiration, in conjunction with the general debility and want of power in the organs of circulation, the result of the violent and continued muscular spasm, and want of due nourishment by food.

For the following case I have to thank my friend Dr. Rogers, Physician to the West London Hospital. Though it proved fatal, like the previous case, no post-mortem examination could, unfortunately, be obtained.

CASE 33.—*Tetanus following laceration of the hand.*

Joseph R—, a hard-working man of intemperate habits, was admitted into the General Hospital, Northampton, with a severe lacerated wound of the right hand, and a compound comminuted fracture of the right forefinger. An attempt was made to save the forefinger by applying a gutta-percha splint, &c., and the other wound was dressed in the usual manner. He progressed very favorably until the ninth morning after the accident, when he complained of feeling low and sinking, and the wound appeared sloughing, and the hand œdematous. Salines and ammonia ordered.

March 16th, 1852.—In the evening he spoke of an inability to open his mouth freely, and slight rigidity of the masseters and sterno-mastoïds was perceived, which gradually became more marked. Calomel and Pulv. Jalapæ were ordered. At 12 (midnight), he took Calomel, gr. vi, Opii, gr. ij, and at 3 a.m. the dose was repeated. At 5 a.m. an enema of turpentine and castor oil was given. At 10 a.m. the pulse was 100, and weak, and the skin very moist. Free incisions were made into the forefinger. Five grs. of calomel and croton oil were given. At 12 at noon the enema was repeated, and about an hour afterwards the forefinger was removed, the patient being under the influence of chloroform, which slightly relaxed the muscles. At 2 p.m. the calomel and croton oil were repeated, and at 6 p.m. a pill was ordered every second hour, containing Ext. Cannab Ind. Quinæ Disulph. and Hyd. Chlor. Port wine and beef tea *ad libitum.* At 8 p.m. the pulse was 88, and soft, and there was profuse perspiration. The bowels were open freely. There was some difficulty in getting him to swallow the medicine. It was not until late on the 18th that symptoms of narcotism made the slightest appearance, the medicine being regularly persevered in.

18th.—His respiration was easy, and the pulse ranged from 88—100. The sweating was most profuse. At 8 p.m. a blister was applied to the spine and on the calf of each leg.

19th, 2 a.m.—There was stertorous respiration, and all medicines were left off. He was ordered only to take support and nourishment. The perspiration increased, if possible, and he became hourly weaker, and died on the morning of the 20th.

No post-mortem examination could be obtained.

CASE 34.—*Tetanus following a gunshot wound of the abdomen.*

The patient was a young man, æt. about 20, living in Malta. His gun went off, and its contents (shot) entered just above the pubes, lodging principally in the muscles. He walked from the country into town, a distance of about seven miles. Tetanus soon came on, beginning with trismus. He died generally convulsed; no delirium existed. There was no protrusion of the bowels.

The treatment consisted of enlargement of the wound, extracting paper, &c., venesection to 2 or 3 pints, opium and musk. The respiration towards the close of life was hurried and laboured.

Post-mortem examination about fourteen hours after death.—The vessels of brain were turgid, the minute vessels injected, especially on the pons Varolii; also on the pia mater of the spinal cord (the upper portion of which alone was examined). The sheath of the eighth pair of nerves in the neck was in spots injected—as it were, spotted with blood; and on the right side, upon division of the nerve, an appearance of blood existed in its substance. The diaphragm was unusually high; both lungs generally adhering and being very much compressed, and a good deal of blood in them. The bronchi and lower parts of the trachea were very red; the gullet pale. There was no marked disease of the stomach. *The right phrenic and sympathetic nerve* (semilunar ganglia) *were examined, but no distinct morbid appearance was found.* The omentum was adhering to the peritoneum, just above the cavity of the pelvis. The peritoneum there was of a leaden hue. In two or three places in the small intestines there were dark discolorations, like the remains of ecchymosis, and small shot were lodged beneath the peritoneal coat, and in situations in which, from their present position, could be least expected, serving to indicate a change of place. The liver was very generally adherent to the diaphragm; the bladder distended with urine. The abdominal muscles below the umbilicus severely wounded and black and fœtid.

Remarks.—The appearance of the par vagum was worthy of notice, and also the injected state of the vessels of the brain, especially at the base.[1]

CASE 35.—*Tetanus apparently resulting from exposure to cold.—Peculiar substance imbedded in the liver.*

The patient, æt. 66, a Maltese boatman, stout and well made, after exposure to a current of cool air when in a state of perspiration, became affected by tetanus. Three days after its commencement he came into the hospital, and was bled to one pint. The muscles of the jaw were principally and first affected; those of the back and loins in a less degree. He died after being in hospital three days.

Post-mortem examination seven hours after death.—Cranium.—The bones of the head (both tables), especially the occipital, were much injected with blood. The dura mater was also considerably injected; the arachnoïd slightly opaque. The pia mater was redder than natural, especially between the convolutions, where was a little serous fluid. A good deal of fluid existed in the lateral ventricles, and the fornix was softer than natural. The pineal gland was very small, and no gritty matter existed in it. Very many bloody points were seen on the cut surface of the brain, and more serum than usual at base of brain. The appearance of the base was very similar to that of the upper surface.

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[1] For this and the three following cases, which occurred now many years ago, I have to thank my valued friend the late Dr. John Davy.

Thorax.—About one ounce of serum existed in the pericardium. Heart rather large; the foramen ovale was closed; there was no distinct disease; but the substance of the heart was easily broken by pressure of the thumb. Some blood existed in the aorta; and a good deal in the right cavities of the heart, much coagulated. The thoracic duct was pretty large and empty. The lungs, adherent in many points to the thoracic walls, were apparently sound, their inferior portions gorged with blood, and consequently dark red. No tubercles existed.

Abdomen.—The omentum had descended through the left abdominal ring into the scrotum, and adhered apparently. No intestine accompanied it; nor was there space for intestine to descend. On the superior convex surface of the right lobe of the liver was an indentation something like a cicatrix, and just above it *a small spherical mass, less than a walnut, composed of cartilage-like substance, partially calcified, and contained in a sac very like perichondrium, was embedded in the liver.*[1] The substance of the liver was apparently sound, the gall bladder distended with bile. The stomach was empty, rather small; it contained in many places red patches and streaks, and was much injected. The inner coat was very soft generally. The spleen was rather flabby, and not very easily cut. Vessels of large size were very numerous in it; and its section in consequence was apparently cellular. The psoas muscles on each side were excessively tender and easily broken; blood was effused above them under the peritoneum, and between the fibres of the left one. The masseter muscles were unusually tender; as also the muscles of the chest and thighs. The penis was rather livid, and marks existed of emission having occurred.

Remarks.—The appearances on dissection were worthy of notice, and especially the soft state of many of the muscles, particularly of the heart, and the blood effused amongst the muscles of the loins.

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[1] For the description of a somewhat similar body, which I have described as having been imbedded in the surface of the liver, see 'Trans. of the Pathological Society,' vol. vi, p. 208.

CASE 36.—*Tetanus following fracture of the fore-arm.*

Charles S—, æt. 30, a soldier in Malta, was admitted into hospital with swelling of the lower part of the right forearm and severe pain in the back.

When drunk, on the previous night, he fell over a part of the rampart, said to be thirty feet high. Only six ounces of blood could be abstracted by venesection, and an aperient was given. He was then bled to two pints and cathartics repeated. On the day after admission he complained chiefly of the back, the pain being increased by a full inspiration. The pulse was full and strong; the swelling of the wrist rather less. He was bled to two pints, and again purged. Much relief followed the bleeding. Still he complained of the back, and motion increased the pain. One dark stool only was procured. Pulse 88. The skin was cool. Castor oil was given. Three days after admission he had a bandage to support the loins. Four stools followed; and the back was much easier. In the evening were slight febrile symptoms, and the arm was more swelled and painful. Calomel and Pulv. antimonialis, of each gr. iv. were given. On the day afterwards several stools were passed, and the arm and back were better. Five days after admission he had a restless night, and there was general pain and uneasiness; the pain of the back was now of a shooting kind. Pulse about 100, and respiration rather quick. He bled a little from the nose. Skin cool; stools scanty. Calomel and Antimony, of each gr. iv. were ordered twice a day, and castor oil. In the evening, on getting out to close stool, he had occasional severe pain of back with impeded respiration, and was bled to two pints: relief from several copious stools. On the day afterwards he had had a restless night, and slight rigidity existed about the lower jaw. Laudanum ʒj was given; relief followed. Less pain of back. Pulse 120. Skin warm and moist. The powder and anodyne draught were given when necessary, and wine allowed *ad libitum*. In the afternoon pain in the belly came on, and an enema was given with relief. More rigidity about the neck, and occasional pain at the scrobiculus cordis came on, and he had the anodyne, and subsequently an emplastrum lyttæ was applied to the pit of the stomach. On the seventh day after admission he had had a restless night, with frequent, but not very severe spasms. The anodyne this evening. Jaw and neck less rigid since; some difficulty in swallowing anything but liquids existed. Much thirst. The pulse was 120. Spasms (momentary) came on about every five minutes. The wrist was less swelled; wound healing. Twenty drops of laudanum were given every two hours, and at 3 p.m. spasms were not so frequent, but more severe, affecting the abdominal muscles and the left leg. The stools were passed in bed. Before seven the convulsions became greatly aggravated. After a severe but short struggle he expired (seven days after admission).

Post-mortem examination sixteen hours after death.—The body was well formed, stout, muscular, and fat. Skin of recumbent parts very livid, from infiltration of blood.

The head was not examined.

Left hand and arm very much swollen. On dissection blood was found extravasated in the sheaths of the tendons and in the cellular structure. The inferior extremity of the radius was fractured, and its articulating surface exhibited lines of fissure of irregular figure, &c.

Thorax.—The lungs generally adhering, contained a tolerable amount of blood and serum. The bronchi were redder than natural. The right side of heart distended with air and blood. The great vessels, both venæ cavæ and aorta, contained much blood. Fibrinous concretions existed in the heart and great vessels—firmer in the descending than in the ascending vena cava. The coats of vessels stained red, more in some places than in others, chiefly where in contact with blood. No tubercles existed. The intestines were rather distended with air. A part of the ascending colon in the right iliac fossa was of almost gangrenous appearance. Ecchymosis in the parts below. Its inner coat was here rather stained; its glands very large.

Spine.—A good deal of ecchymosis existed in the left iliac region, and two transverse processes of the vertebræ of the lumbar part of spine were fractured. Blood was found effused under the theca of the spine in the lumbar region, and its pia mater appeared redder than natural.

Remarks.—In this case the extravasated blood in the theca of the medulla spinalis near the fractured processes of vertebræ was deserving of notice. Venesection here was pushed, but the relief from it was only momentary. Putrefaction in an incipient state occurred very early, and perhaps the air in the heart was owing to this cause.

CASE 37.—*Tetanus apparently caused by exposure to cold.—Lumbrici in the intestines.*

The patient was a stout, well-made, muscular porter, æt. 28, a native of Malta, who, having been subjected to exposure to cold winds of three days' duration, became affected by tetanus, affecting chiefly the muscles of the jaw and back. He was bled and blistered, but died shortly after admission.

Post-mortem examination twelve hours after death.—The body was well formed and muscular; limbs contracted and rigid.

Cranium.—The bones were very thick, and the impressions of the vessels deeply marked. The convolutions of the brain were rather flattened. A tolerable amount of serum between the membranes. The vessels of the brain were rather turgid generally. Very little fluid existed in the ventricles.

Spinal column.—The bones were natural. The spinal cord was also natural. The left lung and the middle and inferior lobe of the right lung were gorged with blood, as in pulmonary hæmorrhage. Bloody fluid existed in the bronchi, which were deeply stained red. Frothy blood without coagulum in great vessels and cavities of heart. There was no well-marked appearance of disease of the abdominal viscera. Stomach, perhaps, rather redder than usual. Large intestines distended with air. *Seventy* round worms of different sizes, most of them dead, existed in the small intestines. The eighth pair of cranial nerves in the neck was pale; *also the sympathetic nerves. The latter in the chest were redder than natural, but this might be the effect of stain from the gorged lung in contact with it.*

For the following case I have to thank my friend Mr. Sedgwick.

CASE 38.—*Traumatic tetanus.*

M. A. H—, a pale and delicate-looking girl, æt. 13 years, who, in consequence of the poverty of her parents, had been living as servant with a coachman's family in a mews near Cavendish Square. The place she slept in was very damp, so that her clothes sometimes became mildewed. The coachman's family consisted of six children, and her work was in consequence very hard, as she had to make herself generally useful, and to carry out the youngest child. It appears that the coachman's boys often beat her, and on one occasion especially, December 14th, 1851, the day on which she was taken ill, one of the boys, aged fourteen years, whose shoes she had neglected to clean well, struck her very hard over the back of the head and neck with a stick, and she fell down. After this beating she could not stoop from pain and stiffness in the back, and her parents, therefore, took her home. At the time of her removal she had been in the situation only seven weeks, and there were numerous marks of bruises on the arms, back, &c., of former as well as of later blows.

I was not called in to see her till January 2nd, 1852, and I then found her lying on the right side in bed, with the body curved forcibly backwards from tetanic spasm. She was quite conscious. Exacerbations of tetanic spasm came on in quick succession, and she complained almost incessantly of agonising pain in the back of the head and neck, which were so intense and unbearable as to make her long for death. Great tenderness was felt on pressure over the spines of the cervical vertebræ. The pupils were natural. The pulse was very quick and difficult to count. The power of swallowing was to a great extent lost, for, on giving her a little water, she would make an effort to swallow it, which brought on spasm, and the fluid was very quickly thrown out of the mouth. For some days previously the bowels had been costive, and she had apparently lost the use of the lower limbs. She lived on in great suffering for three days after my first visit, and retained consciousness till within a very short time of her death.

The treatment was as follows: the head was shaved; and twelve leeches were applied to the nape of the neck; spirit lotions, and a brisk purgative; small doses of calomel internally, and mercurial inunction.

Post-mortem examination, twenty-three hours after death.—The brain and spinal cord were examined with great care. The upper part of the theca of the cord was much injected. The medulla oblongata, with the upper part of the spinal cord, were found on section to be somewhat (but not much) more vascular than usual. On the under surface of the pons Varolii, close to its junction with the left crus cerebri, was a firm mass of fibrin, about the size of a haricot bean, with three small nodules of the same in the vicinity. They were of a pale straw colour, semi-transparent, and moderately firm on pressure. The viscera of the chest and abdomen seemed healthy on superficial inspection.

Though I have purposely only recorded such cases of tetanus as proved fatal, I think the details of the following successful case which I watched will be read with interest.

CASE 39.—*Tetanus apparently resulting from exposure to weather.*

James H—, æt. 18, a labourer, was admitted into St. George's Hospital August 17th, 1861, suffering with tetanus. His parents and their family (including himself up to the date of this attack) had always enjoyed good health, as long as he could remember. He was exposed to the weather for years previously to this attack, occasionally getting wet through. In the December of 1860, however, he worked at a gas factory, and was exposed to a high temperature. In the January of 1861 he resumed his former out-door occupation, and continued in it, exposed to wet and cold, until the middle of March. Subsequently he was discharged by his employers, and remained at home with his parents in good health for one week, when he was attacked in the manner stated further on. He had never had his hands sore from mixing lime, to which he has been accustomed; never punctured his thumb nor otherwise injured his hands; never trod on a nail nor otherwise injured his feet or any other part of his body; never had worms nor other intestinal irritation. "Had always been contented and happy."

The patient stated with reference to the present attack that it commenced on Sunday, March 17th (four weeks three days before his admission, April 17th). On that day he was constantly gaping, the mouth being open, while simultaneously the lips were drawn backwards and inwards. Shortly afterwards the muscles became stiff, and the mouth could not be opened wider than was sufficient to allow of thin bread and butter passing into it. These events occurred without pain, and the stiffness continued without pain for four days, and during the interval he fed on sop. Subsequently the muscles relaxed slightly, and at this stage he suffered pain at and around the temples. About the tenth day the legs became stiff, then the hips, and two days subsequently stiffness followed by pain became general over the hips, back, and belly, with gradual opisthotonos of the trunk. These conditions persisted until Saturday, March 30th (two weeks), and during the interval he could take short steps, each step being attended with pains in the hips and knees. At the last date he walked about a mile, and suffered great agony from the exertion. On the following day (Sunday, March 31st), the preceding symptoms increased to such a degree that he was obliged to lie on his back, and keep himself constantly stretched out by the muscular actions of his limbs to abate the pain (in some measure) which he experienced in and around the abdomen. Subsequently, he was so much relieved that he was enabled to take short but painful steps, and on admission (April 17th) he walked from a cab at the hospital door to the ward without assistance. After the first week he took warm, sweet beer in the day, and in the evening gruel and rum, to promote sweating. He was always thirsty. He occasionally ate bread and butter, but against his will. For the first week the bowels were inactive, subsequently they were kept open by purgatives.

His appearance was that of a strong but plethoric young man of about five feet ten inches in height, of very florid complexion, broad face, prominent cheek-bones, and pleasing expression. There were broad florid patches on the dorsum of the left hand extending up the left arm. These marks he said were congenital.

The jaws were incapable of being separated further than about one inch and an eighth. The body was rigidly arched from the scapulæ to the nates, the trunk resting on a pillow. Spasms existed, recurring at intervals, drawing the chest upwards and affecting the limbs. The tongue was protruded about three quarters of an inch. The articulation was perfect. Breathing and circulation natural.

The treatment consisted in the internal administration of one sixth of a grain of extract of belladonna at intervals of four hours apart for one day, of three hours apart for the second day, and of six hours apart for the succeeding days, thirteen in number, the period embraced dating from April 17th to May 2nd. The pulse ranged at 70. The pupils were but slightly affected. The bowels were kept regular by castor oil.

On May 2nd he was put upon quinine, and the belladonna discontinued. His diet consisted of solid food as much as he could eat, and his appetite was remarkably good.

At the early period of this treatment he suffered from pain at each inspiration, concurrent with the muscular spasms.

On the 6th of May he was out of bed and progressing, and ultimately recovered.

Remarks.—Notice the constant gaping at the onset of the disease, which began with stiffness of the muscles of the face and then of the jaws.

Postscript.—I recorded Cases 12A and 13 as instances of spasm resembling tetanus, I will now cite the following additional cases in which tetanoïd spasms came on in the course of disease which was not properly tetanus :

Mary W—, æt. 30, was admitted into the hospital, January 15, 1848, with inflammation of the subarachnoïd tissue of the brain, in connection with suppuration and destruction of the ear and caries of the temporal bone. When admitted, in addition to other symptoms, there was dysphagia and complete opisthotonos. Subsequently loss of smell came on. The dysphagia appeared to be caused by the extreme opisthotonos, which continued during sleep. She died three days after admission. [21.]

John K—, æt. 50, was admitted into the hospital, July 23, 1845, quite unconscious, with loss of power in all the limbs, stertorous breathing, contracted pupils, and slight opisthotonos. He died on the same day. After death extravasation of blood was found in the right crus cerebri, right corpus striatum, and in the pons Varolii, especially on the right side of the median line low down. [181.]

Philip B—, æt. 25, was admitted into the hospital, November 24, 1847, with a scalp wound, for which trephining was resorted to. More or less paralysis of the right side of the body came on before the trephining. Sloughing of dura mater, and extensive abscess of brain followed. Shortly before death he became much less sensible, and *the jaws were so firmly fixed as to require some force to be used when it was necessary to open them.* [244.]

Emma F—, æt. 11, was admitted into the hospital, March 22, of the same year, with arachnitis and inflammation of the brain. On admission there was slight strabismus and a degree of opisthotonos. Later on, in addition to other symptoms, there was a very marked and excessive tenderness over the whole body, but especially the back. She died April 5th, but for several days previously the opisthotonos had become very considerable. [74.]

We have recently had in the hospital the following case, to which my colleague, Dr. Fuller, called my attention. It was that of—

John D—, æt. 3, suffering from spinal meningitis. He was admitted Jan. 13 1868, with albuminuria and pain in the abdomen, and vomiting, following scarlet fever. Opisthotonos and symptoms of cerebral hydrocephalus came on. The opisthotonos recurred from time to time until he died, Feb. 24th. After death turbid fluid was found in large abundance in the cavity of spinal arachnoïd, which was thick and injected, and adherent in places. The central ventricles were distended with fluid. [55.]

Cases in which symptoms of trismus and tetanic spasm have come on in connection with inflammatory action, morbid growths, &c., within the cranium might be multiplied. I will here only refer, in addition, to an interesting case of gliomatous tumour of the brain (occupying the right processus é cerebellô ad testes, pons Varolii, &c.), brought before the Pathological Society by Dr. Cayley, in which, along with other symptoms and opisthotonos, repeated attacks of tetanoïd spasms occurred affecting the whole body (see volume xvi, p. 23, of 'Transactions'); and to a case of apoplexy related by myself at page 2 of volume xii of the same 'Transactions,' in which softening of, and extravasation of blood into the pons Varolii, &c., existed. In this case, in addition to other symptoms, frequent attacks of tetanic spasm occurred, with intervals of extreme rigidity of the muscles.

PART FOURTH.

Chronicle of Medical Science.

(CHIEFLY FOREIGN AND CONTEMPORARY.)

REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By ROBERT HUNTER SEMPLE, M.D.,
Member of the Royal College of Physicians, Physician to the Bloomsbury Dispensary, London.

On the Use of Ipecacuanha. By Dr. WEIGERSHEIM.—Dr. Weigersheim has shown that ipecacuanha root may be replaced by emetine, or even, in certain cases, by cold water, as employed in hydropathic practice. Warm water, drunk in moderate quantities, will often produce vomiting. Dr. Weigersheim also alludes to the fact that the smell of ipecacuanha often excites nausea, and that when mixed with atmospheric air it causes, when it is breathed, remarkable oppression of the respiratory organs, such as hoarseness, sneezing, spasm of the glottis, and even spitting of blood and asthma. He therefore proposes the employment of ipecacuanha in small doses. He mixes 12 centigrammes of the root with 8 grammes of sugar (a centigramme is the hundredth part of a gramme, which is equivalent to about 15 grains of English weight), and divides the powder into 12 parts, and orders one part to be taken in water every five minutes until vomiting ensues. Nausea and even vomiting usually supervene after three or four powders are taken, but sometimes only after seven or twelve. If a powder is vomited immediately after being taken, another must be given at once. Dr. Weigersheim has applied this treatment with success as a preliminary remedy in those cases in which formerly every kind of emetic failed; and either by the continued use of the ipecacuanha or the subsequent administration of a full dose the desired result ensued. He also obtained favorable results from these small emetic doses in those cases of cholera in which an emetic was indicated.—*Schmidt's Jahrbücher der Gesammten Medicin*, August 27, 1868.

On the Use of Ipecacuanha in Emetic Doses as a Stimulant, Restorative, Eliminant, and Adjuvant in various cases of Disorder and Disease. By JOHN HIGGINBOTTOM, F.R.S., of Nottingham.—Mr. Higginbottom attributes great therapeutical efficacy to ipecacuan, not only in the diseases for which it is usually administered, but in many others, and he prefers giving it in emetic doses. The operation of ipecacuan as an emetic is to promote a general reaction of the whole system, which is thus brought into a more active and healthy state, without producing any debility. On the contrary, Mr. Higginbottom states that he has often seen its effects in quickly raising the sinking powers of the system even in extreme cases of syncope and hæmorrhage. His mode of administration is to cause the patient to drink about a pint of weak tea or thin gruel, and half an hour afterwards to take half a drachm of the powdered ipecacuan with ten grains of bicarbonate of potash in a little cold water, and in a quarter of an hour afterwards to drink freely of weak tea or gruel. By this method vomiting is induced safely, quickly, and decidedly, and the lining membrane of the stomach is free to perform its natural healthy functions. During the last fifty years Mr. Higginbottom has made it his usual practice, with almost every patient labouring under either acute or chronic disease, first to ascertain the state of the *primæ viæ*, and if these be not in a satisfactory condition, to prescribe an ipecacuanha emetic, to procure full vomiting. Mr. Higginbottom gives a list of diseases in which this treatment has proved beneficial in his hands. In English cholera ipecacuanha is, according to him, an invaluable remedy, not only at the commencement, but in the last and worst stage of the disease. He has been in the habit, ever since 1813, of giving an ipecacuanha emetic in all cases of fever, at the commencement of the disease, with the most beneficial effect; and he has often found that attacks of the complaint have by this means been warded off or effectually checked. In nearly all cases of simple acute erysipelas he commences the treatment with an emetic dose of ipecacuan and aperients, although he also uses the nitrate of silver as an external application. In bronchitis the use of ipecacuanha in the early stage, in emetic doses, has long been known and practised, but Mr. Higginbottom has found the same treatment beneficial in the last stage of the disease, when sinking has come on. In tic douloureux Mr. Higginbottom begins with an emetic of ipecacuan and with aperients, and then gives citrate of quinine and iron. In habitual drunkenness Mr. Higginbottom believes that an ipecacuanha emetic will often remove the desire for alcoholic drinks, and in delirium tremens he gives emetic doses of ipecacuan in preference to the stimulo-narcotic plan of treatment. In cases of exhaustion during the puerperal state the same treatment has been found very beneficial, and even in the debility of old age he thinks that the powers of life have been improved, and life itself prolonged by this means. In puerperal hæmorrhage also Mr. Higginbottom has employed emetics of ipecacuan with success, and he condemns the practice of giving large quantities of brandy in this affection. He gives a number of cases, some of them of a very remarkable and extraordinary kind, in which ipecacuan emetics seem to have proved very efficacious.—*British Medical Journal*, February 13, 20, and 27, 1869.

On the Action of Tartar Emetic in small and long continued doses. By Dr. A. Nöbiling, of Munich.—The experiments made by Dr.

Nöbiling on himself and on some of the lower animals relate not only to the operation of tartar emetic, but to that of each of its constituents, namely, tartaric acid, potash, and oxide of antimony. The experiments made on himself were by internal administration, and by injections into the subcutaneous areolar tissue and into the veins. The general results at which he arrived by these means were as follows:—Tartar emetic in small doses, from a milligramme ($\frac{1}{1000}$ of 15 grains) upwards, gradually excites in healthy persons very disagreeable effects, as oppression and weight in the præcordia, giddiness in the head, irritation of mind, weakness of the limbs, feverish shivering, inclination to sleep, frequent, irregular and full pulse, pale and sunken face, increased collection of mucus in the throat, and painful deglutition. After a longer use of the drug, there arise, together with loss of appetite and painful oppression in the stomach, short smarting pains in the bowels, frequent yawning, difficulty of breathing, and an unpleasant feeling of coldness on the skin of the trunk and on the extremities. The lower part of the body is extremely painful to the touch; the stools, which are at first normal, become gradually irregular, sometimes fluid, sometimes solid, but the urine is not much altered in its chemical characters. By a still longer use and gradually increasing doses of the tartar emetic all the phenomena are increased, the uneasiness is greater, the stools become thin and muco-bilious, the abdomen is swollen and very tender to the touch, the dulness of the hepatic region increased about a finger's breadth downwards, and this region is very painful on pressure, the heat of the skin is everywhere increased, and there is also itching. There is difficulty of swallowing and increased secretion of mucus. Dr. Nöbiling made a series of experiments on frogs, dogs, and rabbits, in order to ascertain the action of the separate constituents of tartar emetic, and for this purpose he compared the results obtained respectively from potassio-tartrate of antimony, ammonio-tartrate of antimony, and sodio-tartrate of antimony, and also from tartrate of potash and tartrate of soda. These experiments seem to prove that the potash acts upon the heart, causing depression of that organ, for it was found, among many other experiments, that while a small dose of potassio-tartrate of antimony caused death in a frog, the same dose of sodio-tartrate of antimony produced no effect. Dr. Nöbiling draws the following conclusions from his researches:—1. There are two independent modes of action in tartar emetic, one on the heart and the other on the intestinal canal. 2. The action of the potash is on the heart, and that of the antimony is on the intestines. 3. Potash has a direct paralysing action upon the heart. 4. The tartaric acid is without any direct action upon the system. He goes on to observe that the effects on the heart and the pulse, and also on the liver, are due to the action of the potash, and that this is the active principle in causing death in fatal cases of poisoning by large doses of tartar emetic. He does not report favorably of the therapeutical employment of tartar emetic, which he thinks objectionable on account of the potash it contains, but he recommends the *ammonio-tartrate of antimony* as an emetic, because in this, as in the sodio-tartrate of antimony, only the action of the antimony is secured, and moreover, this preparation is not decomposed in a watery solution.—*Schmidt's Jahrbücher der Gersammten Medicin*, November 4, 1868.

On the Alleged Narcotic Properties of Narceia. — By Dr. G. FRONMÜLLER and Dr. S. KERSCH.—Although the narcotic effects of narceia have been lately announced by many authorities, as Claude Bernard, Debout, Béhier, Eulenberg, and Erlenmeyer, Dr. Fronmüller denies to the alkaloid the existence of this property. Soon after its discovery by Pelletier, Majendie and Orfila declared it to be inert, according to their experiments on dogs. Leconte observed in dogs, after the administration of narceia, a troublesome cough, and tenderness of the hind legs, but no cerebral symptoms. Dr. Fronmüller employed narceia twenty-two times, in order to procure sleep, care being taken that the alkaloid was pure. It was used nine times subcutaneously as hydrochlorate of narceia, and thirteen times internally as pure narceia. When used internally, and the dose was gradually raised to 20 grains, no narcotic effect was observed in any of the patients, and the breath, the pulse, the heat of the skin, the secretion of urine, and the state of the pupils were unaffected. Only in one case there was more sleep than usual, but no narcotism. After the two largest doses there was, on the following morning, only slight giddiness. Dr. Fronmüller thinks that even a drachm might be given without any result. The subcutaneous injections produced no effect. Experiments instituted by Dr. Kersch, in Prague, on the lower animals, gave a negative result, and thus they confirm the views of Dr. Fronmüller. In none of the animals submitted to experiment was any trace of narcotism observed, either when the narceia was administered internally, or when it was injected into the pleural cavity.—*Ibid.*, January 28, 1869.

On the Action of Papaverin. By Dr. K. B. HOFMANN.—Dr. Hofmann has made some fresh investigations on papaverin, in consequence of its having been recommended as an agent in causing sleep and relaxing the muscles. He himself took the alkaloid in increasing doses up to six grains, and then he experienced a violent hiccough, which ceased in ten minutes, and subsequently an uncomfortable feeling in the pit of the stomach, and great pain in the forehead after eating, but no sensation of weariness or lassitude. In a second experiment with six grains, all the above symptoms were absent, except the oppression at the stomach. The conclusions at which Dr. Hofmann arrived as the result of his experiments were as follows:—1. Papaverin, in reference to its physiological action, stands very low in the rank of narcotic alkaloids, since in the case of none of them used in medicine does a dose of six grains, taken at once, remain inert. 2. Papaverin, in healthy subjects, does not produce, in doses of six grains, the slightest hypnotic effect. 3. It does not relax the muscles, inasmuch as neither at first nor several hours after it has been taken does it produce weariness or lassitude,

&c. 4. It is not a cumulative agent, and produces no after consequences, so that when taken several days in increasing doses it causes neither sleep nor muscular relaxation. It exerts no striking action on the mental powers analogous to that of other narcotics. 6. The diminution of the pulse is very slight in healthy subjects, namely, about four beats a minute. 7. The breathing and the animal heat are very little altered. 8. Papaverin does not confine the bowels, and does not materially affect the secretion or excretion of the urine, or the elimination of water and chloride of sodium. 9. Whether papaverin shares with opium and morphia the property, when taken in large doses, of disturbing digestion, must remain undecided till further experiments are made.—*Ibid.*, December 28, 1868.

On Carbolic Acid as a Remedial Agent. By Dr. W. KEMPSTER, Utica, N. Y.—Dr. Kempster has made a number of experiments in reference to the therapeutic value of carbolic acid, and the results have been very satisfactory. He recommends that the crystallized acid should be employed in preference to the ordinary acid, which is found in the market as a dark-coloured liquid. Dr. Kempster has used it in catarrh where the discharge was very offensive, and it not only removed the fetor, but in the course of two or three inhalations, it changed the character of the discharge, and effected a cure. The proportion was one grain to an ounce of water. In cases of ozæna, nasal polypi, and diseases of the nasal passages with offensive discharge, it has been used with great success. He has also employed it with success as a gargle in scarlatina with ulcerated throat, and he suggests that it might be used with advantage in ordinary cases of sore throat and in diphtheria. In the State Lunatic Asylum of Utica it is successfully used, in the dose of a drachm of a solution of one grain to the ounce, to relieve cases of sluggishness of the bowels accompanied by offensive breath. It appears to possess the power of arresting fermentation, and is, therefore, serviceable in cases of yeasty stomach, attended with flatulence and expulsion of gas; and it therefore seems likely to be useful in cases of sarcina. As an external application the acid possesses valuable properties, and is extensively used on the Continent of Europe as a dressing for various wounds. In the form of ointment, in the proportion of five grains to an ounce of simple cerate, it corrects the odour attendant on cancerous discharges, and when mixed in larger proportions it destroys such parasites as the *Acarus scabiei* and the *Pediculus capitis.* As a remedial agent in skin diseases it seems to possess decided advantages, and Dr. Kempster mentions a case of tinea capitis which rapidly yielded under its use. Dr. Kempster also confirms the opinion generally entertained of the disinfecting power of the acid, and recommends it as a safe and effectual deodorizer. It appears to arrest the development of the lower forms of animal life, it stops the fermentation of yeast, and kills microscopic infusoria and cheesemites. Dr. Kempster found that it also destroyed larger animals, as crickets, mice, and rats.—*American Journal of Medical Sciences,* July, 1868.

On the Medicinal Uses and the Doses of Carbolic Acid. By Dr. Fuller, of Guy's Hospital.—Dr. Fuller made some investigations, four years ago, upon the use of carbolic acid in medicine as an internal remedy; and he then administered it experimentally in various cases at St. George's Hospital. He began by giving two minims for a dose dissolved in a drachm of glycerine and eleven drachms of water; but, as no appreciable effect was produced from this dose, it was raised gradually on alternate days until the patients were either unable to swallow a stronger dose, or complained of unpleasant symptoms. It was found in general that six or seven minims were a full dose, although some men who had been spirit-drinkers could take ten or twelve minims, or even fifteen minims, three or four times a day for many days in succession. The disagreeable symptoms produced by full or over-full doses were chiefly a sense of burning in the throat, and of giddiness and fulness in the head. In health, the only effect which was observed to follow moderate doses was the production of a peculiar greenish tint in the urine, and the disappearance of all deposit of lithates. From this latter circumstance Dr. Fuller was led to think that the carbolic acid might possibly control the formation of lithic acid; and he therefore administered it in gout and also in rheumatism; but it produced no remedial effect, although it rendered the urine clear. It had no effect on the course of typhus, or of typhoid fever, although it was fully tried. It seemed to be chiefly useful in some dyspeptic cases of the fermentative class. accompanied by the copious evolution of gas from the stomach and the evacuation of fetid matter from the bowels. Dr. Fuller thinks that its action in such cases is similar to that of charcoal, and he has found it to operate beneficially even when charcoal fails to relieve.—*British Medical Journal,* Feb. 20, 1869.

On Chloracetic Acid as a Caustic. By Dr. Urner, of Bonn.—The chloracetic acid is a powerful caustic, acting in some degree like nitric acid in this respect. Experiments made by Dr. Urner in practice are of a very encouraging character in respect to its use in such cases as warts, condylomata, lupus, &c. The pain caused by its application was but slight, and did not last long. The chloracetic acid belongs to the class of caustics which penetrate very deeply, and its operation is not inferior in intensity to that of fuming nitric acid. It is adapted for the burning of the smaller and larger neoplasms, as it can be applied both in a greater or less degree of concentration, and only on the spot required, and it does not affect the surrounding parts. Its application is not so powerful as that of many other caustics, whether stronger or weaker, and it excites no great inflammation of the parts: it leaves behind a moderate scurf, which is soon removed, leaving beneath it healthy granulations: the scars formed by it are moderately smooth, and subsequently undergo no great contraction: symptoms of poisoning by its use need not be feared; its operation can always be regulated, and small quantities are sufficient to procure an instantaneous and effectual cauterization. Nitrate of silver cauterizes less deeply than chloracetic acid, the cicatrisation follows more slowly, and its application is more painful. Chloride of zinc and caustic potash cauterize more deeply than chloracetic acid, but their action is more extended, and attended with much greater pain. The best mode of applying the chloracetic acid is by means of a glass rod, or a glass or asbestos pencil.—*Schmidt's Jahrbücher der Gesammten Medicin*, November 25, 1868.

On the Employment of the Neutral Acetate of Potash in Acute and Chronic Gastro-intestinal Discharges. By Dr. Marotte, Physician of the Hôpital de la Pitié.—Disorder of the mucous membrane of the stomach and bowels may take place with or without fever, and may be either acute or chronic. Physiological causes or pathological conditions may influence its degree, its tenacity, or its duration, but do not at all change its nature. The presence of the products of secretion on the visible mucous membranes, as the tongue and the buccal cavity, their discharge by the stomach or the anus, are the characters of disorders of the mucous membranes, as of all the varieties of gastro-intestinal discharges. But in practice it is well to distinguish two varieties, one in which the products of secretion are in such small quantities that the excretion is reduced almost to a nullity, and occurs only at intervals more or less distant, sometimes voided by the stomach, but more frequently, although still rarely, by the anus. In the other variety the products of secretion are so abundant as to be evacuated almost daily, either by vomiting or by stool. It is in the first variety that the acetate of potash affords most relief, and under its use the clamminess of the mouth and the dryness of the tongue diminish; the stools are voided spontaneously from time to time, or are more easily excited by injections which were before inefficacious; the urine becomes less deep coloured, and the feverish excitement, and especially the exacerbations, are lessened. Dr. Marotte's memoir on this subject concludes with the following propositions:—The acetate of potash, frequently employed in the two last centuries, does not deserve the oblivion into which it has almost fallen; for without possessing the active properties of some other medicines, it is often of use, especially in gastro-intestinal discharges, as the author has proved by experience. It appears to possess a direct sedative action on the irritation of nutrition and secretion, which is the cause of these discharges. Its action does not seem to be the consequence of any appreciable phenomenon, and is manifest only by the diminution of all the morbid symptoms. All the forms and varieties of gastro-intestinal discharges are not equally benefited by its use, and it seems to be best adapted for the simple irritation of the secreting apparatus, without complications. It has a special action on the symptom vomiting, whether occurring in dyspeptic affections or in pregnancy; but its action is definitely exerted only upon the vomiting occurring in disorder of the mucous membranes (*état muqueux*). The dose necessary to be given in these cases should not, in the author's opinion, exceed 2 to 6 or even 8 grammes in twenty-four hours.—*Bulletin Général de Thérapeutique*, Nov. 30, 1868.

On the Sanguinaria Canadensis. By Dr. Victor Van der Espt.—The Sanguinaria Canadensis has been employed for some time in various diseases. All parts of the plant possess a certain activity; but in America only the root is used. It contains a large amount of orange-coloured resin, a red colouring matter which affords the so-called *sanguinarin*, and a small quantity of colourless fixed oil. This root, when dried, appears externally reddish brown, of a spongy texture on fracture, the broken parts being at first orange-coloured, and becoming brown in the air. The powder of the root is brownish yellow, of a slightly narcotic smell, and permanently bitter taste; it excites sneezing. In large doses it is a powerful irritant, and in small ones it causes, in the mouth, a sensation of heat, a bitter taste, and increased salivation; and in the stomach it causes irritation, a feeling of heat in the epigastrium, and improvement of the appetite and of digestion. It acts on the heart and vessels, increasing the circulation. In larger doses, as half a gramme to a gramme, it causes copious vomiting and a general feeling of weakness, and in larger doses still all the above-mentioned symptoms are increased, and there follow gastrodynia, general weakness, prostration, and finally death. Dr. Van der Espt has found that it acts locally as a stimulant to indolent ulcers, and that internally it is a useful remedy in general weakness, especially after exhausting diseases, when the stomach requires a stimulant. It may often be used with success in small and often-repeated doses in acute bronchitis and pneumonia, when it acts like tartar emetic, and in hooping-cough as an emetic. It is especially recommended by American physicians, as well as by Dr. Van der Espt, in diphtheria and croup, especially at the onset of the disease, and in the first instance as an emetic. It is also successfully employed as an emmenagogue.—*Schmidt's Jahrbücher der Gesammten Medicin*, Nov. 25, 1868.

On the Action and Therapeutical Uses of Bromine and its Preparations. By Dr. H. PLETZER, of Bremen, and Dr. OZANAM.—Dr. Pletzer has employed the bromide of potassium in twenty-five cases of epilepsy or epileptiform affections, and he has drawn certain conclusions to the following effect as to its operation. The energy of the heart's impulse diminishes, and the frequency of the beats is lowered, the number of pulsations being generally reduced to 50. Depression of spirits, exhibited by sleepiness, giddiness, want of memory, and weakness of mind, was unmistakeable; but it disappeared on the discontinuance of the drug, or the diminution of the doses. Whether these effects are exclusively due to the bromide, or are common to other salts of potassium, must be determined by future researches. In the motor system large doses caused more or less incapability of spontaneous movements, or a disinclination for it. The motor nerves of the spinal cord appear to experience a paralyzing influence from the bromide. Dr. Pletzer found the temperature in two cases sink one or two degrees. He never observed any appearance of gastro-enteritis, but often slight gastric catarrh and tendency to diarrhœa. In other cases, in spite of one- to two-drachm doses, there was obstinate constipation. Sneezing and slight catarrh of the upper air-passages occurred in some cases, and slight dyspnœa in others, after doses of one to two scruples. The beneficial operation of the bromide in abnormal excitability of the generative organs is unquestionable. In opposition to other observers, Dr. Pletzer has but seldom found the urine to be increased; and even where it was increased he has not always found albumen present. The recommendation of the bromide in diphtheria is unsupported by any valid proof; but as a remedy for convulsive affections and increased reflex irritability it surpasses all other drugs.

Dr. Ozanam, in a treatise 'On the Employment of Solvent and Disaggregating Remedies in Pseudo-membranous Exudations,' recommends bromine in such cases; and he ascribes to this drug, in opposition to the opinion of Pletzer, very successful results. He has made a number of experiments to show the action of different solutions on false membranes, when removed from the body; and, according to his results, it appears that, with the exception of the ammoniacal oxide of copper, which holds the first rank, the alkaline preparations are preferable to the acid ones in pseudo-membranous exudations, and that bromide of potassium stands nearly at the head of the list, being inferior only to phosphate of soda and chlorate of potash, which last has so long been considered the most efficacious. If remedies are sought for their "disaggregating" properties, the series will be as follows:—Chloride of bromine, bromine, and chlorine; then iodine, chloride of iron, corrosive sublimate, and chromium. Dr. Ozanam prefers bromine to all other remedies, because, like bromide of potassium, it has at the same time an influence over the mucous membrane of the throat, the uvula, and the larynx; and, like chlorine, has the power of destroying contagion. For internal use, a watery solution should be employed, in the proportion of one drop to twenty-five or thirty grammes, and of this solution one or two grammes should be given every hour. Fumigations of bromine may also be used in croup; and Dr. Ozanam thinks that very successful results have followed this treatment, and that the extension of epidemics of croup has been prevented by its adoption.—*Ibid.*, Aug. 27, 1868.

On the Action and Therapeutic Value of the Bromide of Potassium. By J. H. Bill, M.D., U.S. Army.—Dr. Bill was induced to make trial of the bromide of potassium in certain cases of onanism, occurring in some music boys of the United States army, in consequence of his having seen a notice of the treatment of nymphomania by the same salt. He treated five cases, twenty grains a day being given; but the results were perplexing, for although at first the remedy seemed to do good, it eventually failed, and none of the cases were cured. But during the treatment it was discovered that the sensibility of the urethra was greatly lessened, and that catheterization, before unbearable, was easily borne. It was found also that this anæsthesia of the urethra was produced by injections of the bromide, and hence it was inferred that the salt acted locally like copaiba or cubebs. It also appears to act as a local anæsthetic on the pharyngeal mucous membrane; and Dr. Bill has noticed, quite recently, that it produces anæsthesia of the conjunctiva. Dr. Bill does not believe that the bromide is a true hypnotic, and in some experiments on himself and others he failed to observe any such action. But he made a series of researches especially to clear up this point, and the results are communicated in the present paper. The experiments, which appear to have been very carefully made, seem to show that the bromide does not act on the nervous centres, but only on the peripheral extremities of the nerves distributed on the mucous membranes. Admitting that bromide of potassium will sometimes cure epilepsy, it is by no means proved that the salt has any action on the brain or spinal cord, for epilepsy is very often of excentric origin, and the beneficial effects of the bromide may be accounted for when the excentric origin of the disease is seated in the mucous membrane of the alimentary canal, or of the genito-urinary cavities. Dr. Bill concludes from his researches that the bromide of potassium is an anæsthetic to the mucous membranes, and that its supposed hypnotic properties must be referred to that circumstance. It acts upon the nerves of the mucous membranes and depresses their action, but its hypnotic effects are only secondary. — *American Journal of the Medical Sciences*, July, 1868.

On the Value of the Essence of Turpentine in Tympanites and the Nervous Disturbances of Dothinenterite. By Dr. CANTEL.—The essence of turpentine, although much employed in England, has been hitherto but little used in French practice, but Dr. Cantel has lately administered it with success in the tympanites and the nervous disturbances which constitute formidable symptoms in dothinenterite. Dr. Cantel admits that in employing the remedy for this purpose he has only carried out the precepts of the late Dr. Graves, of Dublin, although the cases treated in France were instances of typhoid and not of typhus fever, in which latter Dr. Graves advised the employment of the essence of turpentine. In the first case recorded by Dr. Cantel there was tympanitic swelling of the abdomen, together with obstinate constipation, dyspnœa, and delirium. As other remedies had been employed in vain, Dr. Cantel determined to try Graves's plan, and accordingly he ordered a combination of essence of turpentine and castor oil suspended in water to be given in small doses at short intervals, and embrocations of olive oil and essence of turpentine to be rubbed at the same time into the abdomen. The treatment was so successful that Dr. Cantel determined to employ the same remedy in other cases where the same symptoms presented themselves, and he records the particulars of two instances in which the essence of turpentine was used with equal success. He attributes the efficacy of the essence to its action on the nervous system, and not to its purgative powers. At the same time he gives a caution against the antiphlogistic treatment of dothinenterite, and he considers that his cases would not have succeeded as they did, if he had not adopted a supporting instead of a debilitating system of diet.—*Bulletin Général de Thérapeutique*, July 30, 1868.

On the Use of Oil of Turpentine in Parasitic Diseases of the Head and in Traumatic Erysipelas. By Professor von ERLACH and Professor LÜCKE, of Berne.—For the destruction of the parasites which cause many scalp diseases Küchenmeister has recommended alcohol, which retards the development of spores and fungi, but several experiments have proved that the operation of alcohol does not extend to the fungi vegetating in the hair-follicles. Tincture of iodine acts more certainly than alcohol on the fungous growths developed in the hair-follicles, as in Herpes tonsurans. But it is necessary to shave the whole head, and to apply the tincture repeatedly, until the epidermis has been removed three or four times, and even then there are often disappointments in the cure, which in the most favorable cases requires 102 days. According to Professor von Erlach, painting with oil of turpentine is much more certain and more rapid in its operations in these scalp affections, herpes tonsurans being cured within fifty days, and several cases of mentagra being cured in the same manner within a week. Since according to recent researches traumatic erysipelas is to be reckoned among the infectious diseases, and often resists the ordinary methods of treatment, Professor Lücke endeavours to destroy the infectious matter by a drug which penetrates deeply into the tissues, and for this purpose he paints the affected parts with oil of turpentine. He records a case in which this plan was successfully tried, and he states that it was also successful in eight other cases, the erysipelas disappearing in two or three days, whereas formerly he had observed the most dangerous symptoms to occur in similar attacks. A very striking circumstance was the rapid fall of the temperature, in connection with the disappearance of the symptoms, and this reduction of the temperature was always manifested on the first application, and was the more certain in proportion to the energetic use of the remedy, and therefore it was especially observable after rubbing.—*Schmidt's Jahrbücher der Gersammten Medicin,* January 28, 1869.

On the Successful Use of Oil of Savin in Alopecia Pityrodes. By Dr. PINCUS.—Dr. Pincus has used the oil of savin, in the proportion of from 5 to 30 drops to an ounce of spirit, in eleven persons affected with alopecia pityrodes in the second stage, causing it to be rubbed into the head and a compress moistened with the oil to be applied. By this treatment the daily loss of hair was remarkably diminished, the proportion of new hairs to the fall of the old ones became more satisfactory, and the successful result continued even for two or three weeks after the treatment was discontinued. Dr. Pincus has also found tannin, in the proportion of two scruples to an ounce of grease, to act beneficially on the hair-follicles, but the remedial effect ceased after the ointment had been discontinued from four to eight days. Savin, on the other hand, has the disadvantage of its intolerable smell, so that it is often necessarily discontinued, and it also makes the hair brittle and discolours both the hair and the epidermis. In the first stage of alopecia Dr. Pincus found the bicarbonate of potash very serviceable. In all cases it is necessary that the hairy scalp should remain a long time in contact with the remedy.—*Virchow's Archiv,* xliii; 1868.

REPORT ON SURGERY.

By John Chatto, M.R.C.S.E.

Lachrymal conjunctivitis.—M. Galezowski is desirous of adverting in these papers to some of the complications or consecutive affections in diseases of the lachrymal passages which he believes have not received sufficient attention. Lachrymal conjunctivitis is brought about by the too prolonged retention of the tears in the conjunctival *cul de sac;* and although it is so little dwelt upon in books, he has found it occurring 72 times in 363 cases of conjunctivitis which he has noted.

When the tears are neutral in composition, and not excessive in quantity, their sojourn is not attended with irritation, but when their composition becomes changed this is no longer true. The different analyses of authors show that their composition is not always the same, and the author's own researches have demonstrated that it may undergo change according to individuals and morbid dispositions. He always tests this composition by inserting turmeric or litmus paper between the lids, and in the normal state the tears are nearly neutral. In affections of the lachrymal passages they are usually strongly alkaline.

Lachrymal conjunctivitis may be observed under either the chronic or acute form. The first is the most common, and may persist for months or years. This is sometimes transformed suddenly into subacute conjunctivitis, presenting all the appearances of catarrhal conjunctivitis, with which it is, indeed, usually confounded by authors, although really differing from it by the mechanical nature of its cause—obstruction, or abnormal condition of some portion of the lachrymal passages. This remedied, the consecutive conjunctivitis ceases, and awaiting this, cold applications to the eyelids cause great alleviation of suffering, while tepid applications, so useful in catarrhal conjunctivitis, in this form aggravate it. Collyria of sulphate of zinc are also useful.

Under the influence of this same cause a disturbance of vision may arise that is a source of great annoyance to the patient, and is often confounded by the practitioner with a congested condition of the deeper-seated membranes of the eye. Photophobia is a most marked symptom, especially in the presence of artificial light; and what is really only a lachrymal photophobia is often treated for affections of the retina or choroid. The globe is also sensitive to touch, and its movements are attended with pain, while the spasmodic contractions that hence ensue may, by the compression of the globe which they induce, give rise to flashes of light and true phosphenes. The permanent accumulation of the tears confuses vision which is very often accompanied by the presence of *muscæ volitantes,* due to the presence of detached epithelial scales or microscopic objects suspended in the tears. In some cases even diplopia is present when the patient looks at minute objects. All these symptoms may be met with even in a slight degree of obstruction of the lachrymal passages, and are, in some cases, all the more readily confounded with those due to internal affections of the eye, inasmuch as weeping, usually supposed to be characteristic, may be absent. Exploratory injections of the passages elucidate the true nature of the case, and by repetition give great relief, sometimes, indeed, becoming curative.—*Gazette des Hôpitaux*, Sept. 15 and 29.

On Cystitis in the Female.—M. Demarquay observes that this is a common affection, although it is generally but little studied. Those, however, who take the trouble to examine the urine of patients who apply to them on account of diseases of the uterus or its appendages will soon convince themselves of the fact. He is frequently consulted, both in private and hospital practice, by young women who suffer much in voiding the urine, and on examination he finds slight redness of the urethral mucous membrane, with or without tumefaction. Several of these patients had been submitted to various modes of treatment without effect, some even having been subjected to cauterization for the relief of pains supposed to be neuralgic. The pains occur under two circumstances—either when the urethra itself is inflamed, or when the urine is changed in its condition and contains pus. These last cases are due to a chronic cystitis, and the urine placed in champagne glasses deposits a certain amount of muco-pus.

The intimate relations connecting the uterus and bladder leads to the latter becoming influenced by the affections of the former. In acute diseases of the uterus, every one is aware of the more or less considerable disturbance in the emission of urine which takes place, but in chronic affections of the uterus the condition of the bladder has not excited sufficient attention. M. Demarquay has met with several cases of chronic metritis complicated with chronic cystitis, which have been completely misunderstood, which the more easily happens as the patients themselves rarely draw attention to the fact of the pain in passing urine, and the frequency with which it has to be voided. Usually the cystitis yields to the treatment adopted for the metritis when this proves successful; but still, in some cases, women in whom the uterine affections seem perfectly cured still continue to complain of suffering. The exploration of the uterus does not explain this, but on exploring the bladder and examining the condition of the uterus the cause of the suffering is explained. In fact, under some circumstances, most difficult to determine, a cystitis which originally arose from the proximity of the diseased uterus persists with obstinacy long after the latter has been effectually cured. This coincidence of acute or chronic cystitis with acute or chronic disease of the uterus has been noticed by various surgeons, but M. Demarquay does not believe that its frequency is sufficiently acknowledged.—*Union Médicale*, October 3.

Pathological Anatomy of Stumps.—M. Chuvel in this paper presents the results of thirty-two examinations he has made at the Hôtel des Invalides among the pensioners dying during the last two years, and who had formerly undergone amputation. They consisted of eleven amputations of the thigh, eight of the leg, six of the arm, one of the forearm, one of the hand, four disarticulations of the shoulder, and one ante-scaphoidean disarticulation.

The form of a stump having only one bone always tends to become conical, this disposition being far less observable when there are two bones. On first examining it the most remarkable thing is the great abundance of dense, firm, fibrous tissue enveloping the end of the bone, and attaching in a common bond of union the vessels, nerves, and muscles to the cutaneous cicatrix—supplying to some extent the great loss of substance ensuing on the operation. The appearance of the stump, however, much depends upon the general condition of the patient. If he were the subject of some diasthetic affection it participates in the atrophy, while if he were in good condition the stump is plump, having a thick layer of fat between the skin and the degenerated muscles. The form and position of the cicatrix is not usually indicative of the kind of operation that has been performed, but is very dependent on whether or to what degree there has been union by the first intention, or whether this has been unusually delayed. Even in similar operations it varies greatly in appearance, and usually adhering to the bone by short, firm bundles of fibres, these sometimes become much elongated and may be even accompanied by a bursa. In almost every case the *muscles* are found to be atrophied and to have undergone fatty degeneration. This is met with in every degree from that in which the microscope is required for its detection to complete fatty substitution. This has not always depended upon the length of time that has elapsed, but also upon the state of health and of the freedom of the circulation. Every muscle is not thus transformed, and in five of the cases examined, new attachments having been formed with spicula of bone or with the fibrous tissue, the normal condition of the muscles was preserved. The condition of divided *arteries* has been much investigated, but chiefly in relation to the ligature, which here is not in question. M. Hutin states that the obturating coagulum is still found years after amputation, and this was the case in nine out of the thirty-two stumps, the plug being white, hard, and fibrous, without a trace of organization. In fourteen stumps the coagulum had been absorbed and the vessel transformed into a fibrous cord. In fifteen cases the artery was greatly diminished in size. In one it was so notably thinned as to resemble a vein, and in four it was notably thickened. In eight the artery continued permeable to the extremity, its calibre remaining undiminished in four of these. Atheromatous degeneration has not been observed with unusual frequency. M. Chauvel has not met with the fibrous cord prolonged from the artery to the cicatrix described by Notta.

The anastomoses of *nerves* described by Larrey and denied by Hutin have not been observed in these cases; and the tumefactions of their extremities, described by all authors, were absent in six, amputation in all these having been performed above fifty years ago. The tumours were observed in all the others, though in very variable degrees, and minute a description of these is furnished, which we cannot transcribe. Traumatic neuroma is the designation applied to this condition by Cruveilheir, and microscopical examination proves its correctness. An analysis of M. Chauvel's cases shows that pressure as from apparatus does not act as the exciting cause of the neuroma to the extent supposed, for it may be produced when this is entirely absent. The inflammatory irritation subsequent to the operation is the immediate cause, although subsequent pressure or friction may increase the size of the neuroma. This prolonged irritation gives rise to a hypogenesis of connective tissue which constitutes the neurilemma, the nervous fibres themselves undergoing no alteration. The condition of the *bones* depends upon whether the amputation has been one of contiguity or continuity. In all the four cases of disarticulation of the shoulder the glenoïd cavity was found deprived of its cartilage, flattened, or even convex, and covered with fibrous tissue which was non-adherent to the cicatrix and easily removed from the bone, which it left smooth and without traces of inflammation. After amputation of the thigh, especially at its upper third, the effects of inflammatory irritation became apparent in the hypertrophy of the end of the bone by the addition of new layers or the formation of bony stalactites. The statement of Larrey that when a bone is not irritated by pressure it gradually atrophies is scarcely exact; and it would be more exact to say that such bones retain their normal size or exhibit slight atrophy in a few. On the other hand, the influence of the pressure of the wooden leg by which Larrey explained the club-like enlargement of the end of the bone has been exaggerated, for this is found where crutches have alone been employed, and does not always exist when pressure has been made by the wooden leg. The conical form of the stump in amputation of the thigh exposes the bone also to the action of external influences. The new bone deposited is very compact and of an ivory hardness. There is no line of demarcation between it and the old bone, and the whole mass becomes covered by surrounding fibrous tissue through which vessels penetrate.—*Archives Générales*, March.

Pruritus Cutaneus of the Meatus Auditorius.—Dr. Gruber draws attention to this affection, which he says is either overlooked or treated too summarily in treatises on diseases of the ear. Itching of the passage is often met with during the progress of affections of the ear attended with inflammation, or oftener at the end of these when desquamation is set up; but this soon passes away. Eczema of the meatus is a severe affection, but yields sooner than eczema in other parts of the body, at least as far as the itching is concerned. The affection indicated in this paper, however, is the pruritus cutaneus of Hebra. Of this the intense itching of the meatus constitutes the only subjective or objective symptom, the lining of the passage exhibiting no trace whatever of any changed appearance. It is true that in aged persons suffering from it the passage may be found very dry, without a vestige of cerumen, but that this is no essential feature is seen in other cases in which the secretion is found in excess. The pruritus is oftenest met with in persons of middle age and especially in those in whom there exists some disturbance of the circulation. Thus, persons suffering from hæmorrhoids or large varicose veins seem very liable to it, although even children are not exempt. The itching often comes on periodically, *e.g.*, in the evening, the patient having been completely free from it during the day. Oftentimes it may be absent for months, to return again with distressing severity. In the cases seen by Dr. Gruber, the affection has been strictly local, no other part of the body participating in the irritation. Sometimes, as the result of constant scratching, artificial eczema, inflammation, &c., may be set up. As a palliative during the attack a few drops of some watery or oily fluid, as glycerine or almond oil, may be dropped in with good effect. The more habitual sufferers should be taught to pencil the passage with these, or ointments such as the *crême celeste;* and those whose night's rest is liable to be disturbed should, on going to bed, introduce lint imbibed with these substances and leave it in. As a more radical means Dr. Gruber has derived much advantage from the daily pencilling the meatus with a strong solution (gr. x ad ʒij) of the nitrate of silver. This should be done in a good light so as to avoid the membrana tympani, and must not be continued after signs of reaction appear. When this has subsided, if the itching still continue, the pencilling must again be resorted to.—*Allgem. Wien. Med. Zeit.*, December 29.

Summary.

Aneurysm. —Verneuil Case of Spontaneous Popliteal Aneurysm in a Diabetic Subject. (Gaz. Hebd., Jan. 2. Successfully treated by flexure after failure of compression. Attention is drawn to the limit which the existence of diabetes places on our choice of means of treatment.)—Rouge. Aneurysm of the Right Common Carotid successfully treated by Digital Compression. (Gaz. des Hôp. The compression was applied during 130 hours, extending over seventeen days.)—Porter. On a New Arterial Compressor. (Dublin Journal, Feb.)

Bone.—Bergmann. On the Pathological Elongation of Bones (Petersb. Med. Zeit., 1868, No. 2.)

Bronchocele.—Lücke. On Iodine Injections in Parenchymatous Bronchocele. (Berlin Klin. Woch., Dec. 28. Confirmatory of his former statement of the advantage of this practice. *Vide* Brit. and For. Med. Chir. Rev., 1868, p. 558.)

Club-foot.—Sayre. Report on Treatment of Club-foot without Tenotomy. (Trans. Amer. Med. Ass., vol. xix, p. 347. Several cases given with illustrations.)—Quimby. New mode of Treating Congenital Talipes. (Ibid., p. 390. Consists in the application of slips of adhesive plaster, which should be resorted to when the infant is a few weeks old.)—Annandale. Treatment of Congenital Talipes Varus. (Ed. Med. Journ., Feb. Woodcut of the boot employed, tenotomy usually preceding its use.)—Gosselin. Clinical Lecture on the Tarsalgia of Adolescents. (Union Méd., Feb. 13. This name is given to the affection known by others as *valgus douloureux*; and the author believes that the muscular contraction is dependent on an osteo-arthritis. Tenotomy is only required in the later stages—rest and position sufficing in the early ones.)

Dislocation.—Sonrier. Case of Separation and Subluxation backwards of the Malar Bone, with Exophthalmia. (Gaz. des Hôp., Dec. 29.)—Pravaz. On Congenital Dislocation of the Femur. (Lyon Médical, January.)

Ear.—Von Tröltsch. Anatomical Observations on Otorrhea (Arch. f. Ohrenheilkunde, Band iv, H. 2. An elaborate paper, detailing many post-mortem examinations of cases in which otorrhea constituted one of the symptoms.)—Politzer. On a Procedure for Keeping Open Artificial Perforations of the Membrana Tympani. (Wien Med. Woch., 2nd and 5th Dec.)—Gruber. Treatment of Catarrh of the Cavity of the Tympanum. (Gaz. Méd., Jan. 2nd. By the injection of Astringents through the Eustachian tube, for which minute directions are furnished.)

Erysipelas.—Lücke. On the Employment of Oil of Turpentine in Traumatic Erysipelas. (Berlin Klin. Woch., Nov. 9. States that he has derived great advantage from this application, the progress of the erysipelas being promptly arrested, and a remarkable fall of the temperature at once taking place.)

Excision.—Spillmann. On Excision of the Ankle. (Archives Gén., Feb. An able critical appreciation of recorded cases, but adding nothing from personal experience.)—Boeckel. On Excision of the Hip-joint. (Gaz. des Hôp., Jan. 28th. Strongly recommends the operation in hip-joint disease attended with suppuration.)

Eye.—Mannhardt. Clinical Observations on Diseases of the Eye, as observed at Constantinople. (Arch. für Ophthal. Jahr., xiv, Abt. 3.)—Von Graefe. Additional Observations on the Peripheric Linear Incision. (Ibid.)—Stilling. On the Theory of Glaucoma. (Ibid.)—Arlt. Treatment of Diseases of the Lachrymal Passages. (Ibid. Illustrated by engravings of anatomical preparations of the passages.)—Hirschberg. Case of Sarcoma Iridis. (Ibid. With illustrations.)—Laqueur. Clinical Observations on Glaucoma. (Annales d'Oculistique, Jan. Relating to 268 cases observed in Liebreich's clinic.)—Berlin. On Extirpation of the Lachrymal Sac. (Ibid.)—Steffan. On Herpes Zoster Ophthalmicus. (Ibid. Confirmatory of Mr. Hutchinson's observations.)—Leber. On Optic Neuritis. (Ibid., December.)—Galezowski. On Optic Neuritis and Perineuritis in Relation to Cerebral Disease. (Archives Générales, Dec. and Jan. With diagrams.)—Cohn. Condition of the Eyes among the Breslau Literary Men. (Berlin Klin. Woch., Dec. 14th. In continuation of observations on the eyes of school-children, students, and metal-workers.)—Schweigger. Treatment of Stricture of the Lachrymal Duct. (Ibid., Nov. 23. Chiefly by a modification of Bowman's probes, enabling them to be changed only every three or four days.)—Wecker. On a Simplification of the Operation for

Artificial Pupil. (Gazette Heb., Feb. 26th.)—Loring. On the Tests of the Inefficiency of the Recti Muscles. (Boston Journal, Dec.)

Fracture.—Nodet. Three Cases of Compound Fracture of the Ankle-joint. (Lyon Médical, Jan. 15th. Illustrating the advantages of expectation.)—Wulff. On Fractures of the Pelvis. (Petersburg Med. Zeit., 1868, No. 6.)—Chabert. On an Undescribed Variety of Fracture of the Glenoïd Cavity. (Rec. de Méd. Mil., Jan. Occurred at the upper part from a fall on the shoulder.)

Hernia.—Clinical Case of Oblique and Direct Inguinal Hernia occurring on the same side. (Edinb. Med. Journ., March.)—Ravoth. Treatment of Hernia in Infants. (Berlin Klin. Woch., Nov. 16th. Relates a case of strangulated scrotal hernia in an infant ten weeks old successfully treated by operation. On the strength of 200 cases of hernia treated in young infants, Ravoth strongly cautions against the omission of applying trusses on the ground of difficulty and non-necessity.)

Hip-joint.—Hennequin. On Continuous Extension in Coxalgia (Archives Gén., Dec., Jan., and Feb. An elaborate Prize Essay on the Apparatus most suitable to it.) Eve. Hip-joint Operations performed during the late war. (Trans. Amer. Med., April, vol. 18, relates to twenty amputations with four recoveries and thirteen excisions, with five recoveries.)—Sayre. A new Operation for an Artificial Hip-joint, in Bony Anchylosis. (New York Med. Journ., January. Two Cases related, which were originally published in 1863.)—Boeckel. On Excision of the Hip-joint. (Gaz. des Hôp., January 28.)

Invagination—Schott. On Invaginatio Recti. (Jahrbüch. fur Kinderheilk, 1868, H. 4. In relation to its pathological anatomy.)

Laryngoscope.—Navratil. Treatment of Morbid Growths from the Larynx. (Berlin Klin. Woch., Nov. 30, Dec. 7 and 14. Woodcuts.)—Fournié. Exploration of the Pharyngo-nasal Region. (Gaz. des Hôp., Jan. 21. Directions given for accomplishing this, which it is a misnomer to term rhinoscopy.)—Balassa. On the Utility of Division of the Larynx in facilitating the Removal of Morbid Growths. (Wien. Med. Woch., Nov. 11. Relates four cases in which this was done without any subsequent injury to the voice.)—Schrotter. On the same subject. (Allg. Wien. Med. Zeit., Jan. 19 and 26. A case related at some length.)

Ovariotomy.—Maslowsky. On the Seroso-plastic Procedure in Ovariotomy. (Centralblatt, Dec. 26.)—Harris. Case of Successful Ovariotomy after free discharge of Fæcal Matter from the Wound. (Dub. Journ., Feb.)—Kimball. Four cases of Ovariotomy. (Boston Journ., Sept. 17.)

Ozœna.—Cousin. Diagnosis and Treatment of Constitutional Ozœna. (Bull. de Thérap., Dec. 15.)

Plastic Surgery.—Buck. New Method of Reconstructing the

Lower Lip. (Trans. Amer. Med. Assoc., vol. 19. Two cases of Epithelioma given, with Illustrations of the Operation and its Results.)—Hueter. Four cases of Rhinoplasty. (Berlin Klin. Woch., Jan. 2. With Observations and Illustrations.)—Billroth. On Direct and Indirect Osteoplasty. (Allg. Wien. Med. Zeit., Nov. 17 and Dec. 8.)—Menzel. Osteoplastic Experiments. (Wien. Med. Woch., Nov. 25 and 28. Giving an account of experiments performed with Billroth on the influence of the periosteum in the regeneration of bone.)—Richet. On Perinéoraphie by a New Procedure (Union Med., Jan. 16.)

Subclavian Artery.—Report on Ligature of the Subclavian Artery. (Trans. Amer. Med. Assoc., vol. xviii. Tables given of 157 recorded cases. It results from Mr. Poland's researches that many of these were examples of ligature of the axillary artery).

Tetanus.—Wunderlich. Two Cases of Recovery from Tetanus. (Arch. für Heilkunde, 1869, No. 1. One traumatic and the other idiopathic. Observations on the great elevation of temperature observed, and on the agency of aconite in the treatment.)

Ulceration.—Adelmann. Clinical Observations on the *Ulcus perforans plantæ pedis.* (Prag. Viertel, 1869, B. i. Besides a critical review of the sixteen French cases that have been published, the author relates four which have been met with in the Dorpat Clinic.) —Flora. Observations on the Yemen Ulcer. (Allg. Wien Med. Zeit., Dec. 15. Numerous cases observed during the author's residence in Egypt.)

Urinary Organs.—Van Buren. Treatment of Stricture. (New York Journal, Dec.)—Jüngken. Nature and Treatment of Stricture. (Deutsche Klinik Dec.)—Alling. On Injecting Hydrochlorate of Morphia into the Bladder. (Bull Therap., Dec. 30. Relates cases observed in M. Guyon's Wards at the Necker, in which the practice proved useful in the relief of painful affections.)—Demarquay. On Vaginal Cystocele. (Bull Thérap., Jan. 30. Relates a case exemplifying the efficacy of a caoutchouc pessary, of which a woodcut is given.)—Thiersch. On the Mode of Origin and Operative Treatment of Epispadias. (Archiv der Heilk, 1869, No. 1.)

Uterus.—Koeberle. Successful Extirpation of a Fibro-cystic Tumour from the Uterus. (Gaz. Hebd., Feb. 26 and March 12. The tumour weighed fourteen and a half kilogrammes.)

Varicocele.—Eve. On an Operation for the Radical Cure of Varicocele. (Trans. Amer. Med. Assoc., vol. xix. Consists in excising a portion of the scrotum, and securing the most enlarged veins around pins by means of animal sutures.

Varicose Veins.—On the Ligatare of Varicose Veins. (Ibid., vol. xviii. A successful case given in which nine silver ligatures having been passed around the vein, this was completely depleted by incisions in the intervals. A woodcut.)

REPORT ON MIDWIFERY.

By Robert Barnes, M.D. Lond., F.R.C.P.

I.—The Non-Pregnant State.

1. *On Incisions of the Cervix Uteri.* By Thomas Skinner, M.D.
2. *Large Multilocular Ovarian Blood-cyst removed by Operation from a Patient labouring under Peritonitis.* By. A. Wiltshire, M.D.
3. *Imperforation of the Hymen, with Accumulation of Blood in the Vagina and Uterus.* By Dr. Perrin.
4. *Case of Total Extirpation of the Uterus.* By Dr. Grub.
5. *A Case of Inversion (Chronic) of the Uterus.* By James Hakes.
6. *Clinical Researches on the Direction of the Uterus in the Adult Woman.* By M. Panas.

1. Dr. Skinner, discussing the practice of incising the os uteri, says the operation admits of being divided into—1, the major operation, which involves incising the os uteri internum as well as the *os tincæ;* and, 2, the minor, by which the os tincæ only is incised. He prefers excising from within outwards, and this by means of Routh's two-bladed hysterotome. He thinks incision should be preceded by dilatation by metallic sounds. He contends that "on no account ought the vaginal portion of the cervix to be divided or split through and through." He doubts if incision is often serviceable as a cure for dysmenorrhœa, but says the incision of the external os is useful in sterility.—*Liverpool Med. and Surg. Reports*, Oct., 1868.

2. A woman, æt. 50, having an ovarian tumour, was seized suddenly after unusual exertion with acute pain and swelling of the tumour, with vomiting, and other signs of shock. As her only probable rescue from death lay in the removal of the tumour, extirpation was performed. Acute peritonitis was found. The tumour contained a large quantity of blood. Parts of the tumour were very friable; the pedicle broke down under the ligature, and as the only means of commanding the stump, it became necessary to transfix the right half of the uterus, and to tie it with a stout silk ligature. The patient perfectly recovered.—*Trans. of Pathol. Soc.*, 1868.

3. Dr. Perrin relates two cases of imperforate hymen with menstrual retention. Both were relieved by simple incision of the hymen. Charrier at the same time related a case in which incision of the hymen was followed by escape of a white, half-fluid mucus only. Examination revealed, a short distance above the hymen, a hard muscular diaphragm, which he punctured with a trocar. A little blood flowed. Puncture was then made above the pubes, when black, fluid, odourless blood flowed. This was only palliative. The abdomen remained of the size of seven months' pregnancy. Maisonneuve then

punctured by vagina; this time with better result; the opening was maintained for several months, and chlorine washings were used every day on account of the offensive smell. Menstruation returned, and the patient recovered.—*Arch. Gén. de Méd.*

4. A woman, æt. 67, was suffering from carcinoma of the uterus. In July Dr. Grub removed by hand a mass of cauliflower-like substance, and drew it out of the uterus, and, cutting it away by scissors from its connections, extirpated it. The following day the woman buried her uterus, using the spade herself. She died on the 3rd September.—*Monatsschr. f. Geburtsk.*, Dec., 1868.

5. A primipara, æt. 23, was delivered naturally on the 4th March, 1867. A moderate flow of blood followed the expulsion of the placenta. An attack of secondary hæmorrhage set in at about a month afterwards. The doctor who came to see her introduced his hand and arm into the vagina, and brought away an immense quantity of clotted blood. From that time a sanguineous discharge persisted. On examination a tumour was found in the vagina. The os uteri formed a ring round the neck of the tumour, so that the finger passed through this contraction at every part into a *cul-de-sac* about half an inch deep. The taxis, under chloroform, made no impression. Continuous pressure with an air-pessary was then applied for about a fortnight, at the end of which time the tumour was found to be gone, the vagina very much distended, and the os uteri large and pulpy. There was great anæmia. She recovered.—*Liverpool Med. and Surg. Reports*, Oct., 1868.

M. Panas has examined the direction of the uterus in 114 women at the Lourcine who were not affected with uterine disease. He found the uterus quite straight in 44, anteflexions in 40, anteversions in 12, retroflexions in 3, retroversions in 3, lateriversions in 12.—*Arch Gén. de Méd.*, March, 1869.

II.—PREGNANCY.

1. *A Case of Retroversion of the Gravid Womb.* By Dr. V. HASELBERG.
2. *A Case of Retroversion of the Gravid Uterus treated successfully.* By Dr. J. GOODMAN.

1. Dr. V. Haselberg relates a remarkable case of retroversion of the gravid womb. A woman, æt. 38, had borne two children, then two premature labours, then a child at term. From her first pregnancy she had an enormous pendulous abdomen, so that she had to carry it suspended by a belt hung over her shoulders. Being pregnant, retention of urine appeared. The os uteri could not be reached behind the symphysis, but it was at last made out projecting forwards behind the abdominal wall. Attempt at reposition failed, but premature labour and expulsion of the fœtus followed. The uterus retained its abnormal position notwithstanding. The fundus was

quite thrown back in the sacrum, coming close to the vulva, whilst the os was pointed upwards above the symphysis. By lifting up the fundus, and bringing the axis of the uterus into relation with that of the pelvis, the placenta was easily removed. The woman had a strongly angular scoliosis of the thoracic vertebræ to the right. This so shortened the abdominal cavity that the uterus could not find room during pregnancy. This case proves that a complete retroversion of the gravid uterus may exist after the fourth month, without dilatation of the anterior wall into the abdominal cavity. The pregnancy here was in the fifth month.—*Monatsschr. f. Geburtsk.*, Jan., 1869.

2. In Dr. J. Goodman's case of retroversion a previous unsuccessful attempt had been made by the hand in the rectum. Dr. Goodman replaced the uterus by means of the colpeurynter placed in the rectum.—*American Journ. of Med. Science*, Oct., 1868.

III.—Labour.

1. *Clinical Report of the Royal School of Obstetrics of Milan.* By Dr. Casati.
2. *The Harmless Breech-Forceps of* Dr. Ruhstral.
3. *On the Construction of the Cephalotribe.* By Dr. Matthews Duncan.
4. *A Kyphotic Transversely Contracted Pelvis.* By Dr. Th. Hugenberger.
5. *Case of Rupture of the Uterus during Parturition.* By Dr. Peter Young.
6. *Bifid Uterus: pregnancy; labour; hæmorrhage; peritonitis; death; autopsy.* By M. Bailly.

1. Dr. Casati publishes the fifth of his series of 'Annual Reports of the Milan Lying-in Hospital,' of which Professor Lazzati is chief. It contains the usual statistical summaries, and a special description of the results of the treatment of eclampsia by subcutaneous injections of atropine, morphia, and quinine. Five cases are reported; in three injections were used; symptoms of atropine action were developed, but the fits were not arrested. Casati thinks the method may prove useful as an auxiliary, but that we must rely upon antiphlogistic treatment and the emptying of the uterus.—*Annali Univ. di Med. Milano*, 1868.

2. Dr. Ruhstral describes a breech-forceps, and a case in which the instrument was used. It is 15¼ inches long; the blades measure to the lock 9 inches; they have the head-curve of the obstetric forceps corresponding to the width of the breech. The closed forceps has a space of 2¼ inches at the widest part of the bow. The ends which touch are 11 lines broad, and provided with a border turning in about ⅛th of an inch, serving to prevent slipping off. In using the instrument the blades must be applied only in the bend of the thighs.—*Monatsschr. f. Geburtsk.*, Jan., 1869.

3. Dr. Duncan discusses the relative merits of the Paris model of the cephalotribe and of the Edinburgh model, by which it is presumed is meant Sir James Simpson's. From experiments and study of the mechanical properties of the two forms, he concludes that the Paris model crushed the head much more completely; that the blades came more closely together; that it is, in short, much more powerful than the short or Edinburgh model, the blades or handles of which are apt to bend under their work. Dr. Duncan has made some modifications in the Paris model, substituting the Brünninghausen lock for the French, diminishing the pelvic curve, improving the screw, and lessening the weight of the instrument.—*Edinb. Med. and Surg. Journ.*, Dec., 1868.

4. Professor Hugenberger, of St. Petersburg, describes with great minuteness a transversely contracted pelvis, the result of kyphosis. The subject was twice delivered by forceps of a live child. She died of the second labour, of puerperal fever. After a critical history of this form of distortion, he compares his pelvis with the normal standard, and with the similar pelvis described by Moor (Zurich 1865), and by Birnbaum ('Mon. f. Geburtsk,' 1860). The deformity produced showed—1st, increase of the conjugata vera, which exceeded the transverse diameter by 27 mm. 2nd, diminution of the transverse diameter, being 5 mm. less than the normal. 3rd, both sacro-iliaco-cotyloïd or oblique diameters were contracted to 4·4 mm. below the normal. This gives for the inlet a long oval, pointing behind and forwards, for the cavity an irregularly generally contracted funnel, and for the outlet a transverse contraction. Out of 11 labours in 4 individuals affected with kyphotic transversely contracted pelvis, in 5 the forceps was applied, in 1 labour was induced, in 1 craniotomy was performed, rupture taking place. Only 3 labours proceeded normally.—*St. Petersb. Medic. Zeitschr.*, 1868.

5. A woman having had a natural labour, when at term again had protracted labour from unyielding cartilaginous os uteri. Laceration occurred through the vaginal portion of the cervix, extending to the lower part of the uterus, not involving the peritoneum. There was some hemorrhage, but no other marked symptoms. Child was extracted by turning. Woman did well till the fifth day, when, having been disturbed by nurse, profuse hemorrhage occurred, causing death.—*Edinb. Med. Journ.*, Jan., 1869.

6. Dr. Bailly's case of bifid uterus. The patient died in the service of M. Depaul, at the Clinique. The placenta had adhered partly to the septum between the two uterine cavities. This septum being unable to contract, like the normal uterine walls, was, M. Bailly thought, the cause of the hæmorrhages.—*Bull. de la Soc. Anat.*, 1867.

IV.—CHILDBED.

1. *Researches on the Pulse of Women two weeks before and two weeks after Labour.* By Dr. HÉMEY.
2. *On the Measurements of the Temperature in Lying-in Women.* By Dr. CREDÉ.
3. *On the Infection-Theory of Puerperal Fever, and its Consequences for Sanitary Police.* By Dr. BOEHR.
4. *A Case of Puerperal Eclampsia, in which Transfusion was used, Recovery following.* By Dr. W. LANGE.
5. *Uterine Phlebitis, followed by Venous Coagulation, with Phlegmasia alba dolens.* By Dr. BAILLY.

1. Dr. Hémey made observations on 400 women as to the pulse in childbed. In 65 women out of 400 a retardation after labour was verified down to 60 or fewer beats. The retardation mostly set in a few hours after labour, and then remained equal for some days. The greatest retardation was on the second day. The course of the labour, if natural, had little influence. Every pathological disturbance brings quickly a rise in the frequency of the pulse. In 27 cases of saccharine urine, 15 gave a remarkable rise in the pulse (92—132) without other disturbance; 12 exhibited retardation. The milk-flow during the second to fifth days of childbed caused, in 280 out of 400, no remarkable alteration of the pulse; sometimes it caused a slight interruption to the retardation, but scarcely ever when the secretion was rich and the breasts full. The retardation holds in premature as well as in mature births. After-pains had no influence. Emotions, however, and gastric disturbances soon cause acceleration; in like manner, but very transiently, changes of posture act. Retardation, then, must be regarded as a favorable symptom. What is the cause? From the diminution of tension consequent upon the rapid suspension of the circulation of the blood in the uterus after the expulsion of the child.

In 94 out of 400 cases irregularity and inequality of pulse were found. The irregularity recurred every two or three beats, was observed from the first to the tenth day. Milk-secretion, duration of labour, death of embryo, after-pains, had little influence; but emotions and gastric disorder, by raising the frequency of the pulse, diminished the irregularity.—*Archives Gén. de Médecine*, 1868.

2 Dr. Credé submitted to the obstetric section of the meeting of physicians, &c., held at Dresden in 1868, the general conclusions drawn from observations on the temperature of lying-in women, conducted for several years. The observations were arranged in groups. 1. Normal temperature, never exceeding 38° C. 2. Rising of temperature about the third day, with quick return to the normal, indicates milk-flow to the breasts, cracks of the nipples, slight bruisings and injuries of the genital canal, painful after-contractions. 3. Rising of temperature on the fourth, fifth, or sixth day, with speedy

fall to the normal—generally constipation, at times no material cause, frequently errors of diet and psychical disturbances. 4. Rising on the fourth day, with longer continuance of the increase, generally over fourteen days, then slow return to the normal—complications of the milder puerperal diseases, with disturbances of digestion, or slighter exudations in the neighbourhood of the uterus. 5. Rising on the second, third, and fourth day, lasting for three to four days, then quick return to the normal—diphtheritic deposit on the mucous membrane of the genitals, and slighter endometritis. 6. Rising on the sixth to the eighth day, lasting two to six days, then rapid return to the normal—breaking up and removal of thrombi from the uterine sinuses, the general condition being almost undisturbed. 7. Early remarkable rising of temperature, even in labour, or on the first or second day, long continuance of the rise without remissions —malignant diseases, ending either fatally or in tedious and exhausting convalescence, as endometritis, peritonitis, perimetritis, phlebitis, &c.—*Monatsschr. f. Geburtsk.*, Dec., 1868.

2. Dr. Duroziez has studied the question of the enlargement of the heart under pregnancy on 135 women. Taking the normal area of the heart in non-pregnant women to be 9 centim. high and 12 centim. wide, he arrived at the following conclusions:—1. That the enlargement during pregnancy may be determined. 2. That during the first day of the labour the left cavities of the heart diminish, the right remaining enlarged. 3. That at the time of the milk-flow about the third day the heart becomes elongated; it remains enlarged during suckling, and diminishes when the woman does not suckle. 4. That in women who have had many children the heart is larger than in those who have had only one or two.—*Gaz. des Hôp.*, 1868.

3. Dr. Boehr, recognising fully the doctrine that puerperal fever is propagated by infection carried by the touch, argues in favour of compulsory sanitary regulations to prevent it.

4. A secundipara was admitted at the Heidelberg Clinique with œdema, albuminuria, commencing labour, and convulsions. Delivery was accelerated by forceps. The child was dead, macerated. She was bled, had ice to head, glyster, morphine injections, chloroform, cold douche to head. After thirty-two fits, being in the highest degree exhausted, in deep sopor, transfusion was resorted to. Blood difibrinated was used; seven ounces was injected. The pulse rose; breathing became freer; cyanosis disappeared; sleep followed. She recovered.—*Prager Virteljhrsch.*, 1868.

5. In Dr. Bailly's case of phlegmasia dolens autopsy revealed general and suppurative peritonitis; a purulent collection in the pelvis behind the pubis; the principal veins of the left leg were completely obliterated by clots at the valvules; these clots were traced in continuity to the uterine branches of the hypogastric veins. Dr. Bailly deduces that phlegmasia dolens always proceeds from uterine phlebitis.—*Bull. de la Soc. Anatom. de Paris*, 1867.

The following, for want of space, are cited by title only:

On the Treatment of Uterine Functional Disturbance. By Dr. J. H. Webster.—*The Practitioner*, Feb., 1869.

Removal of an Inverted Uterus by Écraseur; recovery. By Mr. A. Barker.—*Brit. Med. Journ.*, 1868.

A New Method of Conducting the After-treatment in the Operation for Vesico-vaginal Fistula. By J. Goodman, M.D. Louisville, 1869.—He adopts a peculiar self-retaining catheter, and allows the patient to sit the day after the operation.

Shoulder-presentation; Double Perforation of the Uterus following attempts to turn; Version (afterwards undertaken and effected) by M. Tarnier; death. By M. Jolly.—*Bull. de la Soc. Anat.*, 1867.

Fœtal Heterolopia in form of a Dermoïd Cyst.—Ibid.

Ovarian Cyst; Puncture, followed by hæmorrhage into the Cyst; pneumonia; death.—Ibid. The tumour was generally not very vascular.

Cases of Ovariotomy. By Thomas Keith.—*Edinb. Med. Journ.*, Dec. 1868.—Mr. Keith relates six cases, all ending in recovery.

Examples of Cyst-formation in the neighbourhood of the Female Urethra. By W. O. Priestley.—*Brit. Med. Journ.*, 1869.

Pelvic Hæmatocele appearing after Delivery. By A. B. Steele.—*Ibid.*

Rupture of Fallopian Tube; peritonitis; recovery. By G. G. Stanger.—*Ibid.*

Cases of Extra-uterine Fœtation detected at an Early Stage. By E. Kennedy.—*Brit. Med. Journ.*, 1869.

Experiments on Cow-pox. Depaul.—*Mém. de l'Academie des Sciences*, 1867-68.

Extensive Pemphigus, lasting Three Months, in a Pregnant Woman; complete and rapid recovery after delivery. By Dr. Klein.—*Allgem. Wiener Medic. Zeitung*, 1867.

Case of Cæsarian Section, successful.—New York Med. Journ., 1868.

Diagnosis and Treatment of Uterine Polypi. By G. H. Kidd, M.D. —*Dub. Quart. Journ.*, Feb., 1869.

On Retroflexion of the Uterus. By Lombe Atthill, M.D.—*Ibid.*

On the Nature of Puerperal Fever. By M. Delore.—*Lyon Médical*, 1869.

REPORT ON TOXICOLOGY, FORENSIC MEDICINE, AND HYGIÈNE.

By Benjamin W. Richardson, M.D., F.R.S.,

I.—Toxicology.

On the Unsusceptibility of Pigeons to the Toxic Action of Opium.—Dr. Weir Mitchell records that, in September last, it became desirable, in the prosecution of his researches, to produce profound sleep in pigeons. For this purpose he injected under the wing of a pigeon half a grain of sulphate of morphia dissolved in water. Returning to his laboratory in a few hours, he found the bird unaffected. He then injected the groin of a second pigeon with forty drops of black drop (Acetum opii), and in ten minutes gave by the mouth, through a tube, fifty-five drops. At the same time a third pigeon took sixty drops of black drop inwardly, and at the tenth minute received by injection, in one groin, forty-five drops; and at the fifteenth minute, in the other groin, an equal amount. The latter bird soon became very awkward in its movements; but, as this was possibly due to the irritation caused by the injections in the groins, Dr. M. was not sure that, either in this or in the companion case, he could perceive a single unmistakeable sign of opium poisoning. Astounded at an immunity apparently so perfect, he endeavoured, a few days later, to discover what amount of opium was needed to kill, and obtained the following results:—A pigeon was made to take eighty drops of black drop internally; there was no effect excepting a tendency to keep quiet; no signs of stupor; no change of pupils; the feathers were ruffled, as is common with these birds when sick from any cause. Another pigeon received forty-two minims of black drop by injection under the skin of the groin. The symptoms were the same as in the last case. Neither of the birds slept at all, and both were well the next day. A pigeon received under the skin, in three localities, in all, two grains of sulphate of morphia dissolved in water slightly acidulated with acetic acid. No effects were seen, other than those described in the former cases. A pigeon took, internally, three grains of sulphate of morphia, dissolved. Recovery followed without any notable symptoms. A pigeon took, at 8.30 a.m., 272 drops of black drop. The animal retained the dose during an hour, but at 12 was found to have vomited an unknown amount of it, by estimate at least half. The bird recovered, after remaining all day quiet in the corner of his cage; he was not asleep, was easily roused, and was able to execute every usual movement, as flying, walking, and the like. The final experiment Dr. Mitchell thinks decisive:—To a large pigeon, which within the two preceding days had swallowed forty-two drops of black drop, he gave, between 2 p.m. and 6 o'clock. twenty-one grains of powdered opium in soft pills of three grains each. Except the usual tendency to remain quiet, none of the common evidences of opium poisoning appeared, and the pigeon was well and active the next day.

Most poisons affect the pigeon readily enough; but Dr. Mitchell says, so far as he is aware, no other creature is thus protected from the power of opium. The difficulty of influencing rabbits by belladonna and its derivative, atropia, is well known; and the dog is far less susceptible to the effects of the latter drug than is man. Thus Dr. Mitchell has seen a dog recover after taking twenty-one grains of atropia; but he had, in the interval, violent convulsions. The case of the pigeon seems to stand quite alone in the absence of symptoms, and in the amount of a poison which it can bear unharmed. The explanation of the cause of this escape would, in all likelihood, enlighten us as to the true mode in which opium acts upon the creatures which are apt to feel its peculiar powers; but whether we shall ever attain to a knowledge of the reason of these strange unsusceptibilities Dr. Mitchell thinks is doubtful. The most remarkable of them all is the failure of serpent venom to injure its maker or other poisonous snake, and he has never been able to obtain the faintest clue to the cause of the protection, although he has industriously examined the matter in every conceivable way.—*American Journal of the Medical Sciences*, January, 1869.

[Dr. Weir Mitchell is one of the very few investigators who really observe without prejudice, and with true and earnest simplicity. Enthusiastic in work, he conquers all enthusiasm respecting results, and simply reads what he sees in nature. He is consequently an observer whose statement of a natural fact is as near to proof as can possibly be. But on a point so singular as that which he has stated in the above narrative I did not think it correct to let the statement pass, even on his *ipse dixit*, without appealing to nature for confirmation or denial. I therefore this month repeated his experiments, with all the care I could give.

Into the system of a pigeon, with the assistance of Dr. Sedgwick, I introduced two grains of acetate of morphia in solution of forty minims of water, and a sufficient quantity of acetic acid to dissolve the salt. We injected subcutaneously at three points, viz. under one wing and in the thigh on each side. The injection was steadily and readily effected; and the operation, which commenced at 4.45 p.m., was completed at 5 p.m. It seemed to give no pain or anxiety. At five minutes after five the temperature of the animal, in the cloaca, showed a fall of two degrees F. The action of the heart was also reduced in power, and the breathing was slower and rather laboured; the pupils, at the same time, were unaffected; there was no tendency to coma, and no irregularity of muscular movement. At fifteen minutes after five the animal vomited some peas, on which it had recently been fed. The act was over in a few seconds; but directly afterwards the temperature had fallen two more degrees. For a short time, at most half an hour, the animal, in moving about, seemed inclined to drop its head forward, and its feathers were a little disturbed, but it showed no tendency to sleep; and from this time, except that there was exhibited a little difficulty in moving the legs and the wing on the side which had been injected (all the difficulty being clearly due to the process of injection), no peculiar symptom was observed in the animal—certainly not a trace of narcotism, or other indication of poisoning by an opiate.

In a second experiment I gave to a pigeon twenty-one grains of solid opium, in pills, all in one administration. The animal showed no symptoms whatever. Next day I gave to the same animal thirty grains. No effects followed. Twenty-four hours later I gave it forty grains. Again there was an entire absence of symptoms. Twenty-four hours later still I gave fifty grains in one dose. Once more the animal continued to all appearance unaffected. The pupils were not influenced, the movements of the body were regular, the temperature was not reduced one degree Fahr., and there was no vomiting. I fully confirm, therefore, Dr. Mitchell's observation of the natural fact, although I do not agree with him as to the inexplicable character of the fact.

As bearing upon the same subject I may recall attention to an observation of my own recorded in 1858, in my work on the coagulation of the blood (p. 103). I refer to an experiment in which a young healthy cat received, at one dose, seven drachms of Battley's solution of opium; afterwards ten grains of morphia, in one dose; and still later twenty grains of morphia, in one dose, without being destroyed or rendered unconscious.

The symptoms produced in this animal were profuse salivation, occasional slight vomiting, a half sleepy state with purring and rolling action as if in play, followed, at last, with dislike for fluids, short sharp snapping of the jaw, and other symptoms suggesting even a rabid condition. The animal was ultimately killed with chloroform, administered by inhalation.

The facts thus demonstrated in regard to the inaction of opium as a narcotic on some animals are most important in the physiological study of poisons. They suggest several questions, all of which call for solution.

1st. In respect to birds, it is of moment to know whether the resistance to opium holds good in all birds, and specially whether carnivorous birds exhibit exception.

2nd. It would be well to ascertain whether pigeons are equally insusceptible to the action of other vegetable narcotic agents.

3rd. There is the most curious question of all:—What is the reason that any one animal should escape so potent an agent as opium? It is clear that a pigeon can receive at once as much opium as would produce dangerous or even fatal symptoms if divided equally and administered to twenty children under one year old, the weight of the bird itself being at least fifteen times less than that of each child, or 200 times less than the combined weight of the children. Why does the bird pass unscathed? What is there in its organism which destroys the poison or neutralises its action? I have time only now to suggest these inquiries and if I had more time I could not discuss them here at length. Suffice it for the present to confirm and extend Dr. Mitchell's valuable and accurate observation.

Action of Hydrocyanic Acid on the Medulla Oblongata.—Dr. Joseph Jones, of the University of Nashville, has conducted some exceedingly interesting experiments on alligators, in order to determine the action of hydrocyanic acid on the nervous system. The conclusion he comes to is that the acid acts "primarily, directly, and chiefly upon the medulla oblongata and spinal cord, and that its power to produce sudden death is dependent upon its action on the medulla oblongata." In the young alligator the cartilaginous walls which cover in the cerebro-spinal nervous system are easily removed by the knife, so that the poison may be applied directly to the nerve centre. Used in this way it acts in the same manner as when it is applied to a raw surface of the body, or to the surfaces of the stomach or bowels, but more slowly. The results are the same in either case of administration; to use the author's own words—"derangements in the relations of the medulla oblongata and spinal cord to the muscular system generally, and especially to the respiratory system, are the first phenomena manifested in the action of prussic acid." In warm-blooded animals death takes place almost immediately after inhalation of the poison; when it is taken by the mouth, a sufficient time always elapses for the absorption of the poison and its distribution to the great nerve centres. As soon as the poison reaches the medulla oblongata and the spinal cord, convulsive motions are excited, and if the impression be sufficiently intense there is an immediate arrest of the action of the ganglionic cells presiding over the respiratory processes; upon this the death follows. The acid acts also on the blood and on the sympathetic nervous system, but the disturbance causing death is exerted through the medulla.—*Medical Record*, December 16th, 1867.

Poisoning by Nitro-benzin.—Dr. Kreuser relates a case in which a strong workman, æt. 24, swallowed a small quantity of nitro-benzin by drawing it through a tube from a cask. He continued quietly working for two hours when he was attacked with headache, giddiness, and imperfection of speech. His face became white and livid; he was seized with convulsions and fell senseless. In this state he was brought to the infirmary to which Dr. Kreuser is attached, at 8 p.m., five hours after the accident of imbibing the poison. The pupils of the patient were now dilated, and his pulse was scarcelyperceptible. He often gave utterance to loud cries. There was involuntary passage of urine and fæces, and at eleven o'clock vomiting, the vomited matter containing about a teaspoonful of an oily fluid which had an odour of bitter almond oil. The same odour came from the lungs and skin. He was ordered mineral acid drink alternating with coffee. He now gradually showed signs of quiet, his cold extremities became warm, but his pulse remained weak. The next day his condition was unchanged, except that there was more decided cyanosis. The respiration was calm, the sleep persistent. The pupils contracted well, but there was rotation of the eye-balls from right to left and back again. There was neither convulsion nor hiccup. The temperature of the body was 102°, the pulse 106, weak. Camphor was now ordered, and he was placed on a bed before an open window. Next morning there was profuse sweating, with discharge of froth from the mouth, and the pulse was 136. At 4 p.m. he died. As the pulse and respiration became irregular there was opisthotonos. At the post-mortem, forty hours after death, there was cadaveric regidity, and in all the cavities of the body a distinct odour of bitter almonds. The serous covering of the heart was ecchymosed, and the right heart was distended with a frothy fluid. The liver on the outer and on the cut surface was yellow in colour; the stomach was ecchymosed.—*Wurtemburg Corr.*, Bl. xxxvii, 26.

Phosphorus Poisoning.—Köhler has collated under this head some very curious and instructive cases, each illustrating some special points, of value both to the physiological and medico-legal scholar.

The first of these cases is recorded by Dr. E. Walser, and is given at length. In brief, it is the history of a previously healthy unmarried woman, a servant, æt. 27, who in the night of the 4th of January, after giving birth to a twenty-two-weeks' child (this was her third child), died suddenly and suspiciously. She had for three days been suffering from giddiness, prostration, and pains in the limbs; she had been intensely cold, in remissions, with parching thirst, and had been several times sick. Her mistress, suspecting her state, sent for a nurse, who stated that, from the middle of the day of the 3rd of January, she had removed the vomit, which had an acrid smell (but added that Hofman's drops had been administered); she had not discovered any smell of lucifer matches. To this woman, as well as to the accoucheur, the patient decidedly denied having resorted to any means for producing abortion, and attributed her illness to a violent cold. She had suffered from vomiting in her previous confinements. The birth took place at half-past two in the morning; and feverish heat lasted until death, which occurred at half-past three, as well as continued vomiting of food mixed with blood. Shortly before the birth she complained of sharp pains in the stomach, and afterwards of griping pains, which prevented sleep. She had epistaxis. No phosphorus was discovered, by chemical analysis, in the body after death. The liver was found to have undergone extensive fatty degeneration; the true liver-cells were much disorganized, and concreted leucin and tyrosin were present in the hepatic vessels and peritoneal surface of the liver. There were other indications also compatible with phosphorous poisoning, but the case is doubtfully one of poisoning at all. The suggestion is made, and it is worthy of regard, whether the detection of the presence of leucin or tyrosin in the urine, blood, or other secretions, may be accepted as indicative of natural hepatic degeneration as distinct from that which occurs in poisoning by phosphorus.

A second case is given from Dr. Klett. A girl, æt. 23, on the 8th of June, about eight o'clock in the evening, was found, half undressed, and lying on the bed, in a garret in her master's house: she had all the symptoms of gastro-enteritis. The patient complained of burning pain in the epigastric region and over the whole abdomen and extending upwards into the fauces; there was also choking, vomiting of a peculiarly offensive kind, swelling, and sensibility over the stomach to the touch, dyspnœa and extreme prostration; great thirst, pain, disturbance in the head, giddiness, coldness of the extremities, and retention of urine and fæces. The pulse was 100, weak, and quick. She confessed immediately she was six months advanced in pregnancy, and had eaten the ends of lucifer matches, upon bread, to produce miscarriage. She was treated with magnesia in some gelatinous liquid, with leeches applied to the epigastrium, and other remedies antidotal in character, and was taken into the hospital. At half-past twelve at night a five and a half months' child was born, the after-birth following immediately. In a quarter of an hour afterwards there was oppression, difficulty of swallowing, tremor, and collapse, and at three-quarters past one in the morning of the 9th of June she died delirious, and with symptoms of paralysis. The post-mortem yielded all the signs of phosphorus poisoning. The liver had undergone fatty degeneration.

The third case is by Fournier and Ollivier, of a girl only 14 years old, who had never before been very ill, and was living in tolerably easy circumstances. She had been employed for four hours every day for four years in a lucifer-match manufactory. Although from her occupation she was constantly coming in contact with the phosphorus mixture, and was very careless about washing her hands, she had never, during the four years, suffered, apparently, from the poison, except from pustules on the fingers. Three months before her present illness she had, through carelessness, burnt her left arm very severely, and it was, therefore, on her admission into hospital, covered with a scab. On the 25th of February, 1868, the patient felt a sharp pain and difficulty when masticating, in the middle of the left lower jaw; on the 27th bleeding of the gums came on, together with pain, chilliness, and symptoms of fever, and swelling of the cheek; on the 28th, the swelling became much larger, harder, and painful, and the breath extremely fœtid; whilst a bloody fluid constantly kept flowing from the left corner of the mouth. In this state the patient was taken into the Hotel-Dieu on the third day of the illness. The before-mentioned swelling now extended over the upper half of the left side of the throat. The mouth could only be opened with difficulty, for examination. The gums of the affected side were of violet colour, contrasting strongly with those of the healthy side; the teeth of the same side were painful when pressed, but were not loose. There was falling in of the features, paleness of the skin, such as is only seen in leukæmia, prostration similar to that in typhus, entire loss of appetite for many days, and purple-like exanthema over the whole body, especially on the lower extremities. The pulse was 112. At the base of the heart systolic murmurs were heard. On a sudden an increase of all the symptoms was followed by somnolency, and the death of the patient on the sixth day.

The post-mortem showed no indication of enlargement of the liver, and no fatty change in the hepatic cells; neither were the other appearances strictly characteristic of poisoning by phosphorus.—*Wurtemb. Corr. Bl. xxxviii*, 29 ,1868: *Klett, a. a. O. Nr.* 36, 1868; *Fournier and Ollivier, L'Union Médicale*, 36, 1868: *Köhler, Schmidt's Jahrbücher Jahrgang*, 1869, *Nr.* 2.

II. HYGIENE.

Milk Sickness.—A committee of the Kentucky State Medical Society has reported through the chairman, Dr. Yandell, on milk sickness. The earlier settlers of Kentucky and Tennessee encountered a malady presenting many features dissimilar to any of those observed in the diseases with which they were previously acquainted; they have set it down as a new malady and one peculiar to the rich country they had so recently occupied. From the fact that it appeared nearly always to be traceable to the use of milk or butter, or of beef, veal, or mutton as food, it became a prevalent belief that the disease was produced by a poisonous condition of the milk of the cow and of the flesh of the domestic animals used as aliment, derived from the substances upon which they fed. This led to the name *milk-sickness*, which is still in use. A disease which in the lower animals is supposed to correspond with the milk-sickness was termed *trembles.*

After a careful examination of the leading facts supposed to sustain the popular theory of the causation of the so-called milk-sickness, the reporters conclude as follows:—

"They by no means assert that the poison which originates milk-sickness is not derived from milk, butter, or the flesh of animals impregnated with it, but with the evidence before them they are compelled to regard the proposition as not proved. A vast amount of careless observation has been published on the subject. Many of the statements on record in reference to it are inconsistent with one another. Many of the cases that have been reported as those of milk-sickness are destitute of all the characteristics of the disease. Cases are reported in which the poison was so chronic that it was only by active exercise, after weeks of incubation, that the poison was rendered operative, and other cases in which they were in the act of eating the poisoned food suddenly expired. It is impossible to reconcile observations so contradictory. It is not easy to believe that an agent so deadly in its action as to destroy the life of a calf while sucking the cow may yet be harmless to the latter, and be eliminated, as has been asserted, from her system by feeding on clover; the statement that men have been killed by eating the flesh of oxen which, to all appearance, were in perfect health when slaughtered; or that a woman may be poisoned by a soup made from fowls that had been picking at the carcases of animals that had died of *trembles.*

"It is possible," says the report, "that a plant exists possessed of all the noxious properties requisite to develop in the human system the symptoms of milk-sickness through the medium of the food of cattle feeding upon it, but it cannot be believed that such plant can be perfectly harmless to some members of a family and act as a violent poison on others."—*Transactions of the Kentucky State Medical Society*, 1868.

On Measuring the Intensity of Total Daylight.—Mr. Roger J. Wright describes a method by which the experimentalist may measure, at least approximately, the intensity of total daylight. A rod of solid metal, terminated by a heavy base which keeps the rod in a perpendicular position, is placed perpendicularly. Over the rod a hollow tube is made to fit and slide accurately; and this tube is blackened on the inside. The extremity of the rod is painted of snowy white with a jet black spot in the centre. The rod is marked with a scale beginning with the zero line at the base. The tube is pushed over the rod until the lower extremity of it coincides with the zero line of the rod. The method of using the instrument is as follows:—Draw the tube gently up the rod, at the same time looking steadily at the black spot before mentioned. It will be found as the tube ascends the black spot will gradually disappear and ultimately vanish in the gloom; it will also be found that on *different days* and on *different hours of the same day*, the *point at which the black spot vanishes will vary with the intensity* of the light. This point is read off on the graduated scale, and thus we are enabled to measure the intensity of the light at any required time. In taking an observation it would be well to state whether that portion of the sky round the zenith from which the cone of rays proceeds be clear or cloudy.—*Proceedings of the Royal Society*, vol. xvi, No. 104.

[Mr. Wright's mode of observation might be turned to very useful account in experiments to determine various chemical reactions and changes under the influence of light. It might also be applied to the investigation of the action of some poisons of the organic series.]

Epidemic Disease from Eating Black Puddings.—Kussmaul describes an "epidemic," produced by using as food a kind of haggis composed of the flesh of the cow and of the hog, with spices, &c., which occurred in a Bavarian village (Lohr), and its environs, during the month of August, 1868. More than seventy individuals, all of whom had eaten of the food just mentioned, were attacked with a similar form of sickness, while all those who had not tasted the food, even though they had in some instances partaken of a portion of the very same flesh as that from which the puddings had been made were entirely unaffected. The symptoms which occurred in those who had eaten of the pudding, set in within twenty-four hours after it had been taken. There were nausea and vomiting, pain in the stomach, diarrhœa, fever, thirst, headache, vertigo, watchfulness, and great debility. To these symptoms in some cases there were added pain of the limbs, dryness of the mouth, and a sensation of irritation within the throat. In slight cases these symptoms gradually disappeared within fourteen days; in the more severe cases there came on deafness, delirium, convulsions, and other symptoms of cerebral disease, the case then simulating one of typhus fever. To these symptoms were soon added dilatation of the pupils, great diminution in the secretion of urine, often attended with painful micturition, and in some cases with cramp of the legs. In the more severe cases convalescence occurred at the termination of the third or fourth week. Four of the patients died; a boy two years old, forty-eight hours after he had partaken of the haggis, and three grown persons, on respectively the ninth, twelfth, and fourteenth days after having eaten of it. In two cases in which a post-mortem examination was made, the only evidence of disease presented was catarrh of the intestinal mucous membrane, attended with small erosions. In one of the cases there was also enlargement of the mesenteric glands with a greyish-white infiltration; in the other case slight intumescence of the liver was noticed. From the foregoing description it will be seen that the disease in question could not have been typhus fever, for there was an absence throughout its course of exanthemata, of enlargement of the spleen, &c.; neither can the symptoms be referred to the presence of trichinæ, for no appearance of their presence was detected after the most careful examination, in the food eaten by the patients; neither was any inorganic poison discoverable in the food, nor had it undergone in the slightest extent putrefactive change. Besides this, in none of those who had eaten portions of the flesh from the same animals which furnished materials for the confection of the poisonous food, were any bad effects observed. The supposition that the spices used in the composition of the haggis were the media through which something destructive of health had been introduced is extremely improbable. We must therefore conclude that a deleterious principle, similar to that of certain forms of poisonous sausage-meat, had become developed subsequently to the confection of the food partaken of by the inhabitants of the village of Lahr, in Bavaria.—*Deutschen Arch. Klin. Med.*, October 1868, and *D. F. C. in the American Journal of the Medical Sciences*, January, 1869.

IV.—SUMMARY.

The Poisonous Dyes—Coralline, Rouge, and Coralline Jaune. By MM. Tardieu and Roussin. 'Les Mondes,' Feb. 4th, 1869, and *'Pharmaceutical Journal,' March*, 1868.

[In this paper Tardieu and Roussin report that, having in hand a pair of coloured socks which had caused symptoms of disease by being worn, they treated the articles with boiling alcohol, in which the red colouring matter quickly dissolved. The alcoholic solution yielded an extract, with which many experiments were made. Finding this poisonous to a dog, rabbit, and frog, they proceeded to test the coralline itself, with specimens of which they were supplied by Mr. Persoz, jun., who discovered it in the year 1860, and who placed at their disposal three specimens; the first, a pure coralline, the second the red coralline of commerce, and the third yellow coralline. The coralline, or péonine, is obtained from rosolic acid, which is itself a derivative by oxidation from phenic acid. The solid coralline, dissolved in alcohol, when injected under the skin of a dog, in doses of two and a half grains, produced very serious symptoms, and repeated in a dose of three grains, on the fourth day afterwards, destroyed life three days later. A dose of one and a half grains administered in the same way killed a rabbit in four hours; and less than three quarters of a grain killed a frog more quickly. In the dog the liver had undergone fatty degeneration, and in the dog and the rabbit the lungs appeared as if stained by the colouring matter. M. Roussin dyed a skein of silk with the colouring stuff obtained from the liver and lungs of the animals. This affords a mode of detection of the poison.]

Poisoning by Inhaling Mercurial Fumes. By M. A. Ferrand. 'L'Union Medicale,' No. LII, 1868.

[The author records the case of a woman, twenty years of age, who was exposed to the fumes of mercury in a closed chamber in which she also slept, and in which the process of volatilizing mercury was carried on. She complained of want of appetite and a contraction of the epigastrium; the latter increased during a sleepless night with dyspnœa and hiccough accompanied by cough, and followed by vomiting. On the following morning she had acute mercurial stomatitis, with a breaking out similar to the ulcers in diphtheritis, upon the right side of the mucous membrane of the mouth and tongue, swelling of the right submaxillary glands, and profuse secretion of fœtid saliva. The patient had difficulty in speaking and masticating, and five or six days afterwards was attacked by a feverish eruptive exanthem resembling roseola, which began first in the throat and face, and from these parts spread all over the body. In six days the eruption disappeared without leaving any traces; but she continued under charge for a month, owing to the obstinate and intense pain of the limbs.

Injection of Ammonia into the Veins in Cases of Snake Poisoning. By William à Beckett, 'Australian Medical Journal,' Dec. 1868.

[This letter of Mr. à Becket is valuable in that it includes a very clear and concise account (from the pen of a looker-on, Mr. Arnold,) of a case in which Professor Halford injected ammonia, ten minims of strong liquid ammonia, with twenty minims of water, into the left superficial radial vein of a man bitten by a snake—the "*Hoplocephalus superbus.*" Mr. Arnold states that the man at once rose from unconsciousness and helplessness into full intelligence. The relief of the patient was persistent.]

On the same subject. 'Lancet,' March 30th, 1869.

[The 'Lancet,' referring to the experiments and experiences of Professor Halford, epitomizes some experiments made by Dr. Fayrer, Professor of Surgery in the Medical College of Bengal. Into the veins of a dog bitten by a cobra Dr. Fayrer injected a strong solution of ammonia as practised by Dr. Halford, and he also administered ammonia by the mouth, but the dog died in forty-four minutes and fifteen seconds. Similar results were obtained with pigeons. Dr. Fayrer, on his side, therefore, doubts the antidotal value of Professor Halford's method.]

Poisoning by Impure Carbolic Acid. By Edward Swain, Esq., 'Lancet,' March 20th, 1869.

[Mr. Swain's patient had been an intemperate, and had suffered in October last from an apoplectic seizure, producing right hemiplegia. On the 9th of March, 1869, she received, by an accident, in mistake for a dose of house medicine (salts and senna), nearly an ounce and a half of a solution of impure carbolic acid, which had been used for purposes of disinfection. Immediately her lips and tongue became white; she stood with her mouth wide open, and complained of intense burning pain down the œsophagus into the stomach. Soon afterwards she tottered, became insensible, had stertorous breathing, and in spite of all remedies died a little more than an hour after the poison was taken. The post-mortem revealed, as indications of the poison, congestion of the mucous membrane of the œsophagus and stomach, and in the stomach two groups of spots, each spot characterised by a central black portion, with a surrounding areola of greenish hue. The lungs and heart were healthy; the heart empty of blood. The odour of the acid was most readily distinguished in the liver, spleen, and kidneys, so that Mr. Swain thinks a poisonous dose might at any time be detected by the odour. In the alimentary canal, fluid of a brownish-red grumous character was found as low down as the middle of the ileum. It was observed that decomposition, which had commenced in some parts of the body, was altogether absent in those parts in contact with the acid.]

Atropia as an Antidote to Opium. By Dr. M. S. Buttler. 'Medical Record,' August, 1858.

[Dr. Buttler confirms, he thinks, by an experience, the antidotal value of atropia in opium poisoning. His patient showed the symptoms of poisoning, after being injected subcutaneously with half a grain of morphia, so determinately that he feared she was dead, the respiration having stopped. He wisely set up artificial respiration, and at the instance of Professor Budd, injected one sixtieth of a grain of sulphate of atropia subcutaneously. This was done after the artificial respiration had been sustained *for an hour and a half*, and the patient recovered. The artificial respiration in this case seems to us to deserve the credit.]

On the Composition of Cherry Laurel Water. By C. Umney, Esq., F.C.S. 'Pharmaceutical Journal,' February, 1869.

In this essay Mr. Umney directs the attention of the medical profession, as well as the pharmaceutical, to the different amounts of hydrocyanic acid in various specimens of cherry-laurel water. He shows the acid to vary in amount as 53 does to 100 in different samples of the water.]

The Sanitary Aspect of the Sewage Question. By James Adams, M.D. Pamphlet, 1869.

[Dr. Adams in this work defends, most ably, the water-closet as opposed to the earth-closet system of removing sewage from our towns. The pamphlet comes at a proper time, when one of the most important public questions is calling forth the utmost differences of opinion.]

The Dry Earth System. By Professor G. Rolleston, M.D., F.R.S., and George Johnson, M.D. 'Lancet,' March 20th, 1869.

[Two admirable letters in opposition to the dry earth system, and in favour of proper water drainage.]

Remarks on the Resumption of the Mental Faculties at the Approach of Death. By R. L. Roche, M.D. 'Hammond's Quarterly Journal of Psychological Medicine and Medical Jurisprudence.'

[A paper valuable in relation to every point on which it touches, and very ably written.]

REPORT ON PATHOLOGY AND PRINCIPLES AND PRACTICE OF MEDICINE.

By Francis C. Webb, M.D., F.L.S.,

Member of the Royal College of Physicians, Physician to the Great Northern Hospital.

The Asserted Production of General Diseases by Organised Entities.—Dr. Horatio C. Wood, jun., in an able paper combats the idea that the poisons producing general diseases are living organisms. From it we select the following; in reference to the palmella theory of the origin of ague and the asserted antagonism between quinia and palmellæ, he writes:—"On first reading of the antagonism between quinia and these low plants, there were growing in the window near by three specimens of palmellæ. Masses of these were placed in a solution of the sulphate of quinia of the strength of one grain and a half to the ounce. After the lapse of some weeks no difference was perceptible in the vigour of their growth and in that of those in a vessel of simple water near them; all of the plants are in a very healthy state; the quinia has exerted apparently no influence whatever upon them." . . . . "There are various points in the life history of the palmellæ which make it almost impossible that they constitute malaria, or at least render it so very improbable as to require the most positive proof before it can be accepted. None of the palmellæ are known to be parasitic in the proper sense of the term. Unlike the fungi they do not grow in situations where the light does not have access to them. The reasons for these two facts are obvious: a fungus feeds on organic matter, and is consequently fitted to be a parasite, and is independent of light; whilst the palmella, feeding only on inorganic matters, has to decompose and recompose these into organic compounds by means of its chlorophyllous protoplasm. This it can only effect by the aid of light. How could such plants flourish in the interior of the body?" Again, malaria is notably influenced by frost whilst palmellæ are pre-eminent amongst vegetable organisms as resisting the extremes of heat and cold. They will grow in water at a temperature of from 130° to 160°, and freezing has no effect upon them. Again, vegetable decomposition is necessary for the production of malaria, palmellæ are independent of it. They grow wherever moisture and light may be found. The author discusses the statement of Dr. Salisbury in reference to protophytes, which he believes produce measles, rheumatism, and syphilis. In the case of measles he observes with regard to the theory of its production by a fungus from straw, "the stack, stable, barn, and threshing-floors should be foci of disease. How then do farmers' children so generally escape?" And he mentions that Dr. Woodward has inoculated himself and others with the fungus without any effect beyond the production of an ulcer at the point of inoculation. He adds a number of experiments made by Dr. William Pepper, of the Philadelphia Hospital, which seem to prove that the straw fungus can neither produce measles nor protect the system against the disease. In reference to syphilis the author states:—"Dr. W. Pepper and myself carefully studied a number of chancres at the Philadelphia Hospital. We failed to find any cryptograms therein, but did find various streaks of coagulated mucus and fibrous tissue, as well as granular matters within and without cells, all of which resembled the drawings of the filaments and spores as given by Dr. Salisbury. Since this Dr. Willard has sent me, from time to time, sloughs obtained by the action of caustics on fresh chancres. These I have carefully examined with powers ranging from 400 to 800 diameters, and affirm most positively that there were no cryptograms whatever in them, but that they consisted simply of the usual tissue *débris*." The author maintains in the concluding portion of his paper, that when vibrios and their allies are found in the blood they are present as a consequence, not a cause, of morbid processes.—*Amer. Journal Med. Sciences*, October, 1868.

Progressive Muscular Atrophy, with complete Paralysis of the Extremities; Cure by means of the continuous Current. By Dr. NESEMANN, Magdeburg.—A workman, æt. 19, some months after a normal attack of measles, from which he had perfectly recovered, complained of a weakness in the arms. In three weeks this had increased to absolute paralysis, and in the fourth week he had also paresis of the legs.

On admission into hospital there was found great emaciation of the whole body, caused by atrophy of the muscles, the wasting being especially noticeable in the muscles of the hands, and, most of all, in those of the balls of the thumbs, and in the deltoïds. Voluntary movement was totally lost in the arms, but the hands were not bird-claw like. In the legs there was only paresis, the extensor muscles being most affected. Faradaic contractility of the muscles was everywhere diminished, and was in direct proportion to their remaining volume and degree of voluntary power. Cutaneous and muscular sensibility were normal, as also were the functions of the cerebral nerves, the bladder, and rectum.

The diagnosis of progressive muscular atrophy was confirmed by "*harpooning*" the deltoid muscles.

During the next two months the disease made rapid progress, and as the paralysis advanced atrophy increased, and Faradaic contractility diminished; but respiration, appetite, digestion, and sleep, remained unaffected. During this time Faradization was used to the muscles individually.

Three months after the patient's admission the treatment by galvanism was begun, and the cervical sympathetic was daily galvanized for ten minutes. Slight improvement was noticed after only a week's treatment. Motor power increased in the legs first; but it was not till six months after the commencement of galvanization that the patient could walk even imperfectly. It was nine months before he could use his arms at all; but after a year the improvement was rapid. The muscles grew in bulk simultaneously with their gain in power. Galvanization was discontinued at the end of sixteen months; but improvement continued, and two months later the patient was perfectly well. On his discharge from the hospital a fragment of muscle was again *harpooned* out of the deltoïd. The transverse series of the muscular fibres were well and clearly marked, but here and there some minute oil-globules were seen between, and in otherwise normal fibres. Then, as six months previously, the nerve-fibres, visible in the several preparations, appeared to be perfectly normal.—*Berl. Klin. Wchnschr.*, v, 37, 1868; *Schmidt's Jahrb.*, I, 33, 1869.

Fatty Embolism.—On June 2, 1864, a man was received into the surgical wards of Kœnigsberg who had a fracture of the left tibia, complicated by a wound which however did not communicate with the fracture. The following day he became very weak, and he died comatose thirty-six hours after the accident. At the autopsy nothing was found in the body, as seen by the naked eye, except the lesions of the fracture itself. But on examining with the microscope there were found in the capillaries and small arteries of the whole body (brain, conjunctiva, lungs, heart, liver, kidneys, intestinal villi, muscles, skin) an innumerable quantity of colourless fatty globules. Fat was found especially accumulated in the lungs, so that the majority of the capillaries of those organs had become impermeable to the blood. In consequence of this case F. Busch has been led to examine the subject of fatty embolism in the laboratory of the pathological institute of Kœnigsberg, under the superintendence of M. Recklinghausen. If, after trepanning, the marrow of a long bone in a rabbit is broken up fat is found in the pulmonary capillaries. This fat cannot, as has been supposed by Grohe, have been formed during the last struggles of the animal, for it is equally found in animals rapidly killed. This fat is found some minutes after the operation, and presents the same appearance as is observed in man in cases of fatty embolism following fracture. To examine the mode of absorption of the fat and the vessels by which it is performed, Busch extracted the marrow and injected in its place olive oil coloured with vermillion, which was forthwith found in the pulmonary capillaries. The author shows that this is not merely the effect of the pressure of the injection forcing the coloured oil into the mouths of the open vessels. For if a very feeble pressure is employed, so that the blood of the inferior vena cava contains only traces of vermillion, yet at the end of three hours the pulmonary capillaries will be found to be the seat of a rich fatty embolism. The bloodvessels are undoubtedly the chief of the channels by which the fat is absorbed, but the author has always found particles of vermillion, although in small quantity, in the pelvic lymphatic glands. This absorption was evidently due to the open mouths of vessels produced by the injury. The author relates seven cases of fatty embolism which have been observed at the institute of Kœnigsberg, and analyses forty-three cases which have been published.—*Virchow's Archiv;* and *Gazette Médicale de Paris,* Fevrier 27, 1869.

On Purpura, and its Treatment by Secale cornutum. By Dr. Bauer, of Nentershausen.—Dr. Bauer remarks that, in his practice, he not unfrequently met with cases of purpura both *P. simplex* and *P. hæmorrhagica,* which he considers to be only different degrees of the same malady.

The most common form, *P. simplex* (including the undoubtedly rare *P. urticans* of Willan), affects only sucklings and young children, and was observed at least as often in well-nourished, apparently perfectly healthy children, as in the poor and ill-fed. But this affection is, as a rule, considered to be of no importance, and but few cases come under medical care. The majority of the cases for which professional aid was sought were of a more serious nature, hæmorrhage from the nose, the bowels, &c., being present, and the patients had reached mature childhood, or even early youth. In all such cases the writer, in accordance with the experience of Henoch ('Beitr. zur Kinderheilk.'), found all the usual remedies for purpura apparently useless, but the *Secale cornutum* was of marked efficacy. Already effused blood was not absorbed more quickly than usual, but after from one to three, or at the latest, six days, no fresh effusions occurred. The Secale was given in substance, 8—10 grains, once, twice, or three times daily, in infusion or tincture. It did not prevent relapses; but the writer had, in some cases, again given it after intervals of from six months to two years with the same success as at first.

Dr. Bauer had only once seen death in consequence of purpura. This occurred in a young woman, æt. 22, whose father was healthy and robust, but somewhat intemperate, and whose mother, in early youth very strong, had, later in life, been subject to profuse menstruation. The girl had, from the age of twelve years, been every year for some weeks, and later for some months, especially in the summers, under treatment for *Purpura simplex;* still later there came on, in addition, copious epistaxis, and then profuse menstruation, and, finally, pulmonary and intestinal hæmorrhages. In the summer of 1865, these disorders were associated with the so-called rheumatic pains of *Peliosis hæmorrhagica* (Schönlein), with subcutaneous and intermuscular abscesses in the lower extremities, containing a scarcely purulent, red-coloured fluid; menstruation at last became constant, though, indeed, the discharge was only a thin, reddish-coloured fluid. The patient died with symptoms of intense anæmia, especially of hydræmia. Examination of the body was not permitted.—*Deutsche Klin.*, 35, 1868; *Schmidt's Jahrb.*, I, 34. 1869.

On the Existence of Disorders of the Vaso-motor System in the Limbs in some Febrile Disorders, and especially in Pneumonia.—M. R. Lepine has recorded a number of cases of pneumonia in which he has observed differences of temperature in the limbs of the two sides of the body. It may be suggested that these differences may depend upon some functional alteration of the cerebral hemispheres, and may be analogous, in some degree, to the pneumonic hemiplegias to which Charcot has called attention. But the author observes that the patients had no apoplectic symptom, and that their cerebral condition differed in no respect from ordinary pneumonic patients. Besides the vaso-motor paralysis was clearly not hemiplegic. It never occurred in both members of the same side throughout their whole extent. The vaso-motor phenomena of the limbs are not really more cerebral than the congested cheek; and the encephalon is not the reflex centre of their production. Examination of the medulla in two cases and of the sympathetic in one, and of its anastomoses with the brachial plexus, only showed slight congestion, of the value of which the author had great doubts. The following are the author's conclusions: 1. In some febrile maladies, especially in thoracic affections, and particularly in pneumonia, vaso-motor disorders in the limbs may be occasionally observed. Perhaps they are more common in the aged. 2. The differences of the temperature in the symmetrical portions of the limbs of the two sides may be from one to two degrees, and even more. Between the two axillæ there is generally a difference of some tenths of a degree. 3. In pneumonia the member exhibiting the highest temperature generally corresponds to the affected lung; but there are numerous exceptions. It may perhaps be allowed that the functional disorder may be manifested either by an excess or more rarely by a diminution of heat. 4. The existence of vaso-motor disorder does not make the prognosis more serious. Pathological anatomy does not explain it.—*Gazette Médicale de Paris*, October 31, 1868.

Thrombosis and Embolism in Yellow Fever.—Professor Chaillé records a case of thrombosis in yellow fever, which he believes to be the first ever observed or published. On September 10th, 1867, D. G., æt. 22, was attacked by yellow fever, characterised by the severity of the symptoms and great prostration. Within a week profuse and obstinate epistaxis occurred, followed by bloody abscesses on the arms and legs. From the 23rd to the 27th præcordial pain and oppression, weak, rapid pulse, dyspnœa, and some ascites were observed. Paroxysms of dyspnœa and coughing, with viscid expectoration, rigors and profuse diarrhœa, followed. He died on November 7th. During the latter half of his illness he presented a singularly bloated, swollen, livid appearance, limited to the right side of the face and head, neck, arm, and upper part of the thorax, all of which were very œdematous and darkly discoloured. The habitual decubitus was on the right, but change to the left side did not diminish the local œdema and discoloration. On post-mortem examination the right subclavian and internal jugular veins, from their junction and for some distance of their extent, were found completely occluded, in part by coagulated blood, but in chief by a fluid exactly like pus in appearance. This occlusion extended into the terminations of the external jugular, brachial, and cephalic veins; and the farther from the heart the occlusion extended the more the venous contents approached the appearance of coagula and less of pus. The lungs exhibited considerable congestion and innumerable small abscesses and infarcti, the pleuræ were agglutinated by false membranes, and the pleural cavities contained serum and fibrinous coagula. There had been severe and extensive pericarditis, as shown by effusion and false membranes. There was abscess between the pericardum and diaphragm, multiple abscesses in the kidneys; the liver was congested, and the cerebral parenchyma softened, but not uniformly so.—*New Orleans Journal of Medicine*, July 1868.

The Pathological Anatomy of Yellow Fever.—M. Chaillé states that although post-mortem examinations in cases of yellow fever were prohibited in New Orleans by military authority he succeeded in making a few (less than a dozen) autopsies. One half of the cases examined exhibited the *café au lait* liver considered by Louis to be a characteristic lesion, but which the author thinks frequent but not characteristic. Microscopic examination showed fatty degeneration of the liver but not in an exaggerated degree. Lesions of the solar plexus have been said to be constant; the author, however, states that the tissues around the plexus, as in other dependent parts, may give evidence of hypostasis, but in six cases the nerve tissue itself might have been selected as typically normal. The kidneys were always unhealthy; extreme friability characterised them all; they were enlarged, congested, and exhibited petechiæ or ecchymosis; in two cases the kidneys presented multiple abscesses. The mucous membrane of the stomach is found swollen, softened, eroded, congested, blackened. But this condition is not inflammatory. The softening and erosion are post-mortem changes—the black ramolissement of Rokitansky, which occurs when the gastric vessels are gorged with blood. The only true gastric lesion of yellow fever is vascular engorgement, resulting often in rupture of the capillaries and extravasation of blood; which, mixed with and altered by the gastric secretions, constitutes the black vomit.—*New Orleans Journal of Medicine*, July, 1869.

On the Diagnosis of Meningitis by the Ophthalmoscope.—In 86 cases of meningitis recorded by M. E. Bouchut the following was the relative frequency of the intra-ocular lesions observed with the opthalmoscope:—Congestion of the papilla in 66; œdema of the papilla in 39; dilatation of the veins in 62; flexuosities of the veins in 36; hæmorrhage in the retina in 10; venous thrombosis in 29; false aneurism of the veins in 1; white exudations on the retina in 7; deformity of the papilla in 2; decoloration of the choroid in 1;

atrophy of the papilla in 3; absence of lesions in 3; tubercles of the choroid in 3; choroidian atrophy in 1; vesicle on the retina, 1. In reference to the absence of intra-ocular lesions the author observes that in three cases he found no appreciable lesion of the retina or of the papilla. In two of these the examination was not repeated very often, but in the third it was repeated up to the time of death. It is therefore, he says, certain that meningitis may not give rise to any lesions appreciable by the ophthalmoscope. This may be explained by an absence of clots in the sinuses, or in the meningeal veins, or by the absence of ventricular effusion. The intra-ocular lesions which may be observed in the large majority of cases of meningitis Bouchut classifies under three heads:—1. Lesions of the circulation, viz., papillary hyperæmia; flexuosities, dilatation, and varicosities of the retinal veins; hæmorrhages in the retina. 2. Lesions of secretion—œdema of the papilla and retina. 3. Lesions of nutrition—grey granulations and white patches on the retina, choroidian atrophy, tubercles of the choroid and atrophy of the papilla. The causes of these changes are:—1. Obstruction to the circulation in the sinuses of the dura mater, by which the quantity of venous blood contained in the eye is increased, this mechanical condition being frequent in meningitis. 2. Certain morbid changes in the medulla may, through the medium of the sympathetic produce an hyposthenia of the capillaries in the fundus of the eye, giving rise to papillary and choroidian hyperæmia. 3. The direct action of meningo-encephalitis passing from the inflamed meninges and brain to the optic nerve and to its papilla, which exhibit hyperæmia under the influence so transmitted. The first cause produces passive, or mechanical hyperæmia; the second is paralytic or hyposthenic—the relaxation of the capillaries from defective action of the sympathetic; the third is active—inflammatory congestion extending from the meninges and brain to the optic nerve.—*Gazette Médicale de Paris*, Nov. and Dec., 1868.

A Peculiar Character of the Pulse in Meningitis. By Dr. F. SIREDEY.—In four cases of meningitis, some cerebral, some cerebro-spinal, Dr. Siredey had examined the pulse with Marey's sphygmograph, and had noticed a peculiarity in it which appeared to him to possess some value as a help to differential diagnosis. The pulse-curve showed in its descending portion, and in fact in the first third of it, a shallow undulation, descending steadily during the two latter thirds. Post-mortem examination confirmed the diagnosis of meningitis, and showed the absence of cardiac mischief.—*L'Union*, 95, 1868; *Schmidt's Jahrb.*, x, 32, 1868.

The Physiology and Pathology of Hemi-Chorea.—Dr. Hughlings Jackson, in this paper, maintains the view which he has before advanced, that embolism of parts in the region of the corpus striatum gives a most satisfactory explanation of the physiology and pathology of chorea. He believes—1st. That in chorea nerve-tissue forming the convolutions near to the corpus striatum is in parts diseased. 2nd. That the nerve-tissue there diseased is not destroyed, but is unstable.

3rd. That this local instability is frequently brought about by under nutrition, consequent on diminished supply of blood—anæmia—caused by the blocking of small branches of the arteries supplying the convolutions. He supports these opinions—1, on the ground of the muscular region affected. *a*, the movements are often one-sided. Dr. Jackson does not believe that hemi-chorea is uncommon. and when chorea is bi-lateral one side is usually more affected than the other. *b*, the side affected as in hemiplegia from embolism is generally the right side. *c*, the muscles most moved in hemi-chorea are those most palsied in hemiplegia from destruction of part of the corpus striatum. *d*, the leg suffers less than the arm, as in most cases of hemiplegia. *e*, there is usually greater defect of speech when the right side is affected. *f*, the movements sometimes die away into hemiplegia, as from destruction of nerve-tissue in the corpus striatum, and there is almost always some paralysis. 2. He argues from the nature of the movements. The movements are not jerky movements, such as are seen in locomotor ataxy; they are successions of different movements, and are complex; and these characters point, he thinks, to disease of the higher centres of movements, viz. of convolutions on the hemispherical side of the corpus striatum. 3. From the local internal change. This change is a *minute* one, for its nature has never been agreed upon. Patients with chorea never present changes in the optic discs. In chorea we frequently find a condition favourable to embolism, *e. g.* valvular murmur, rheumatism, or scarlet fever. According to Kirkes, Wilks, and Andrews, vegetations are invariably found on the cardiac valves in subjects who have died of chorea. 4. The author argues from the time of the movements. The fact of the movements proves that the nerve-cells and fibres forming the muscles are not destroyed; but, since the movements are disorderly in association and succession, the nerve-cells and fibres cannot be healthy. The nerve-tissue in chorea is neither destroyed nor unhealthy; it is unstable. The author does not attempt to define instability, but it is a condition caused by defective nutrition.—*Edin. Med. Journal,* October, 1868.

The Variation and Vanishing of Cardiac Organic Valvular Murmurs.—In this paper Dr. W. R. Sanders calls attention to the fact that cardiac murmurs, depending upon permanent diseased conditions of the valves, not unfrequently vary remarkably both in character and intensity, not only at long but sometimes within comparatively short intervals of time. Observers overlook the fact that the blood and force of the heart's action, form variable elements in the production of all endocardial murmurs, and fix their minds too exclusively upon the anatomical lesion of the valves, as if the murmurs were necessarily as permanent as the morbid structures with which they are associated. Dr. Sanders shows that this is not the case (1) with regard to systolic mitral murmurs. He has observed these murmurs when dependent on organic lesion of the valves present very marked variations in loudness and distinctness, and sometimes disappear altogether for considerable periods of time. This is the case when the circulation is in vigour. The author does not refer merely to the disappearance of murmurs towards the fatal termination of heart disease or in conditions of great debility. (2) Aortic valvular disease. Dr. Sanders has never observed the regurgitant aortic murmur when not obscured by other sounds completely disappear, but it varies greatly, and sometimes becomes difficult of detection. With regard to obstructive aortic disease the author is not aware from his own [experience whether fluctuation exist to any extent. He has not found the same variations in the intensity of the murmur. (3) Tricuspid valvular lesions, obstructive and regurgitant. Of these the author infers that variations exist; he gives an instance of variation of regurgitant murmur depending on dilatation:—M. D., æt. 34, suffering from severe general bronchitis, which caused great obstruction to the pulmonary circulation, presented on admission a loud blowing tricuspid systolic murmur, *i. e.* audible over the left ventricle only. Whenever the lung symptoms improved, this murmur disappeared, but it returned again whenever the bronchial secretions re-accumulated, and the respiration became more difficult; and this appearance and disappearance was repeated at several different relapses. There was dilation of the right ventricle, but no valvular disease of any kind found post-mortem. The pericardium was normal. (4) Direct or presystolic mitral murmurs are never functional but pathognomonic of obstruction or contraction of the mitral orifice. The author finds that of all cardiac murmurs this is the most subject to variation. He gives the following instance:—A male, æt 32, presented on admission the *fremissement cataire* preceding the apex beat and the corresponding presystolic murmur in a manner obvious to all present. Next day the murmurs were greatly less distinct, and the day after could not be detected. Some days subsequently, the patient being in bed, the murmur was found distinct, but scarcely had a few students listened to it when it again disappeared. Rising in the bed and then lying down was sufficient to render the murmur audible. It disappeared after a few moments' rest. This is the most striking example he has met with of rapid and striking variations in an organic cardiac murmur. The causes of these variations he believes mainly to be changes in the rapidity of the flow of blood, the relative capacity, or the greater or less distension of the different cavities, the roughness or smoothness of the surfaces and the strength of the heart's action.—*Edinb. Med. Journ.*, January, 1869.

A New Sign of Aortic Insufficiency.—M. Marey observes that aortic insufficiency is one of those diseases which especially allows of certain diagnosis. The diastolic souffle at the origin of the aorta and "Corrigan's pulse" are usually quite sufficient to distinguish it. But still there are cases in which certain aneurisms of the aorta are occasionally mistaken for it. Now in these cases the sphygmograph is capable of contributing important evidence. In aortic insufficiency the ventricular circulation is curiously modified. Instead of after its systole the ventricle filling gradually with the blood from the veins at feeble pressure, it is filled in an abrupt and jerking manner, with aortic blood under strong pressure. Hence the tracings of the movements of the heart instead of presenting an almost horizontal line during the ventricular repose as in the normal state, present a rapid ascent, showing that the pressure is raised very quickly in consequence of the reflux of blood from the aorta. — *Gazette Médicale de Paris*, September 19, 1868.

Contributions to the Histology of the Liver in Jaundice.—O. Wyss observes that if there be a slight degree of icterus due to obstruction of the ductus choledochus or the hepatic duct, or merely to closure of a branch of the latter, a section or the surface of the liver presents a marbled deep or greenish brown colour. These differences of colour are due to the fact that the centre of the hepatic lobule is deeper in colour than the periphery. Microscopic examination shows that the deeper colour of the central part is due to a deposit of biliary pigment diffused or finely granulated in the hepatic cells in the neighbourhood of the central vein. The periphery of the lobules in slight degrees of icterus is altogether, or almost free from biliary colouring matter; the interlobular biliary canals are not dilated and present no change. It is otherwise, however, when there is a high degree of jaundice, the consequence of a prolonged obstacle to biliary excretion—as for example when the ductus choledochus has been occluded for two or three months. The liver then presents an intense olive-green colour, and the periphery of the lobules is, in this case, the seat of the deepest coloration. Microscopic examination shows that not only the hepatic cells contain, as in the preceding case, biliary pigment, but in addition, the finest biliary ducts, and especially the capillary biliary canaliculi are filled and distended with bile. On fine section are seen in the interior of the lobules, and between the hepatic cells, green or greenish brown solid corpuscles, rounded or elongated, frequently presenting ramified prolongations. They are always situated between the cells, and often form a network around them. A fine striated membrane is seen around these corpuscles on section. According to the author these corpuscles are nothing but the capillary biliary canaliculi incompletely distended by bile and become varicose. In the case of icterus which has continued a long time we may find a part of the colouring matter of the bile crystallised in the finest biliary ducts. Besides the meshes of the network of biliary canaliculi there may be seen, between the hepatic cells, irregular corpuscles, red, presenting crystalline surfaces. But the form of the crystals is not regular, and will not compare, for example, with that of the crystals of hæmadoidine. They are composed of bilirubine. Wyss confirms the views of Frerichs in reference to the interlobular biliary ducts. The hepatic cells decompose more quickly in icteric than in normal livers; generally the cells are granular, the nucleus but slightly distinct, and although presenting an accumulation of colouring matter in their interior they are diminished in size. These researches confirm the descriptions given by Gerlach and others of the capillary biliary canaliculi or tubules. —*Virchow's Archiv;* and *Gazette Médicale de Paris*, February, 1869.

Enlargement of the Stomach.—Dr. Hoskins has collected twenty-three cases of great enlargement of the stomach. He gives an abstract of the necroscopic appearances, from which he draws the following conclusions: Obstruction or constriction of the pyloric orifice, or of some portion of the intestinal canal, is, in the majority of cases, the direct cause of dilatation of the stomach; but not in all. Of the twenty-three cases, cancer of the pylorus or of the intestines was only observed in five. In the majority of cases of cancer of the pylorus there is no enlargement of the stomach, because the orifice is not so completely closed that the food cannot pass. Cicatrization at the pyloric extremity of the stomach from the action of corrosives was a cause, present in one of the cases. Cicatrization from ulcers is another; two cases are given. Hypertrophy of the mucous or muscular coats at the pyloric extremity of the stomach, to such an extent as to diminish the calibre of the pyloric orifice (in two cases); cysts obstructing the orifice; distension caused by tumours in the liver, pancreas, &c., pressing on the duodenum and pylorus, are also causes. Large quantities of food and drink, hysteria, and paralysis of the muscular coat arising in diseases of the brain and spinal cord, puerperal fever and peritonitis may produce enlargement of the organ. Lastly, there are cases which appear to be congenital, or arise apparently spontaneously, without assignable cause. —*American Journal of Medical Sciences*, January, 1869.

Enlarged Spleen Complicated with Ascites, both depending upon Varicose Dilatation and Thrombosis of the Portal Vein. By Drs. G. W. Balfour and T. Grainger Stewart.—The authors narrate and comment at great length on a case of enlarged spleen in which peritoneal dropsy was somewhat suddenly developed. The interest of the case is chiefly pathological. The following are the chief points in the autopsy. The viscera of the thorax were pushed upwards; the valves and substance of the heart were healthy; the substance of both lungs was natural. The abdomen contained yellowish serum, and the peritoneum was coated with recent lymph. The left lobe of the liver was atrophied; the capsule was at some points thickened, and at the upper surface, near the anterior margin just above the gall-bladder, the substance was atrophied, so that the gall-bladder projected upwards through the liver. The gall-bladder was altered in form, its walls were thickened and œdematous. The substance of the liver was dense and congested; the ducts were thickened throughout, somewhat dilated and stained with bile. The vena cava hapatica was natural, as were also the tributaries of the hepatic vein and the branches of the hepatic artery. The portal vein at its entrance into the liver was completely occluded with clot. Its walls were thickened but exhibited no trace of suppuration. The clot was pretty firmly adherent to the lining membrane, but could be separated. Secondary recent red coagula extended along the branches of the vein for some distance into the liver. At the entrance they were quite decolorized, not softened in the centre. Close by the porta there was a dense fibrous mass of the size of a small orange, and composed of the aneurysmal dilatation of some of the branches. The walls of the vein were in some parts calcareous, the plates varying in size from that of a fourpenny piece to that of a shilling, and situated mostly in the deeper layers of the inner coat. Some parts also presented the atheromatous change. The dilatations aneurysmal or varicose were situated on the splenic vein. They were in four parts, rounded externally, and connected with neighbouring structures by fibrous tissue, and filled with clots mostly decolorized. The walls of the saccules were entire. They were in truth saccular dilatations of the vein occupied by clot; three of them were of the size of pigeon's eggs, the fourth was somewhat smaller and less perfectly filled. The spleen was enlarged, weighed 3lbs. 6oz. The capsule was thickened and opaque, roughened with old and recent lymph. The splenic vein was dilated throughout, in some parts calcareous, the deposit being in the deeper layer of the inner coat. On section the substance of the spleen seemed dense and fibrous. In some parts there were fawn-coloured spots the result of old extravasation. The Malpighian bodies were nowhere prominent. There was no waxy degeneration. The kidneys were natural. There were many recent and old ulcers in the cæcum and colon. Calcareous depositions and atheroma of the portal vein and its tributaries is very rare, as also are varicose or aneurysmal dilatations.—*Edinb. Med. Journ.*, January, 1869.

Bothriocephalus Latus in Ireland.—Dr. W. Frazer states that tapeworms are of rather rare occurrence in Ireland. *Tænia mediocanellata* was first recognised by himself in that country a few months ago. But the rarest of these entozoa is the *Bothriocephalus latus.* The present is the fifth instance which has been recorded, and it is remarkable, because in all the previous Irish and in nearly all the English cases, the worm could be traced to some continental source; the patient in the present instance, however, had never travelled beyond England and Ireland. With the exception of having been at school in Lincolnshire, the patient had always resided in Ireland. Kuchenmeister observes that this worm shows a preference for lake or marshy districts. The patient in this case resided near a large fresh-water lake, and when in Lincolnshire lived on the borders of the fens. The worm was expelled under the influence of the oil of male fern.—*Dublin Quarterly Journal of Medical Science,* November, 1868.

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The Editor is particularly desirous of having all Reports of Hospitals, Asylums, Sanitary Boards, Scientific Societies, &c., forwarded to him, as also Inaugural Lectures, Dissertations for Theses, Medical and Scientific Addresses, &c.

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